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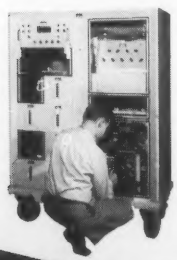

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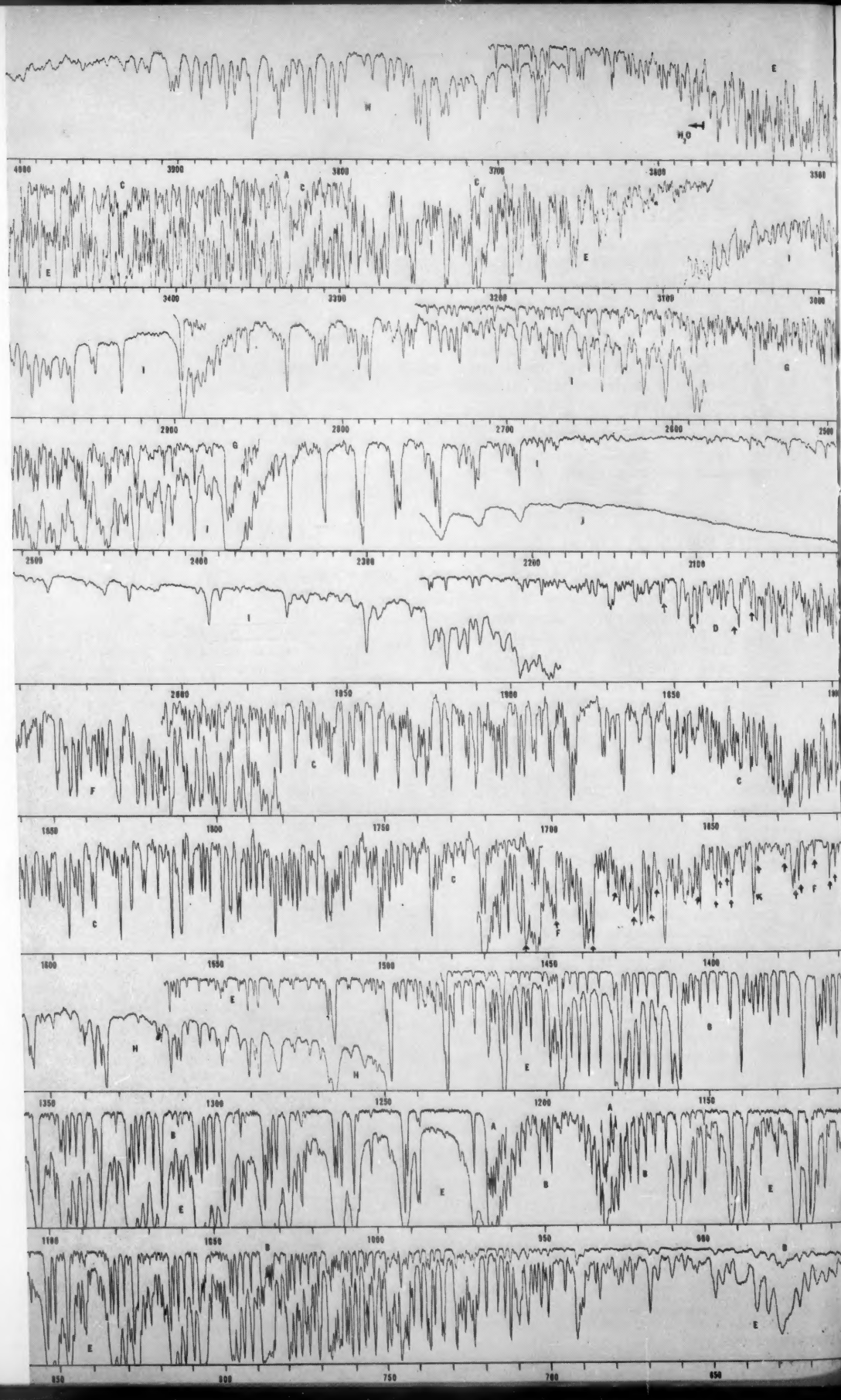
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Letters

Adenine and Plant Growth

The provocative article entitled "Chemical basis for adaptation in plants," by E. B. Kurtz, Jr., which appeared in the 7 November issue of *Science* [**128**, 1115 (1958)] carries a brief description of some work which I did some years ago at the California Institute of Technology. I believe that Kurtz has unintentionally attributed much greater significance to this work than I myself would, and I am eager to have the situation set straight before other individuals draw unduly optimistic conclusions from the brief description in the article.

Some years ago the late Margery Hand and I found that subapical sections of etiolated pea epicotyls grew increasingly well up to temperatures of about 30°C but suffered a sharp thermal inactivation of their growth mechanism at about 35°C. Knowing that certain strains of *Neurospora* were temperature-sensitive adenineless mutants, we decided to apply adenine to these sections in an attempt to reverse the thermal inactivation of growth. This was partially successful, and, in fact, in certain experiments the presence in the culture medium of as little as 5 μg of adenine per milliliter was the difference between life and death for these little etiolated pea sections in an overnight growth test.

In subsequent experiments, described in chapter 25 of *The Experimental Control of Plant Growth* by F. W. Went (Chronica Botanica, Waltham, Mass., 1957), we attempted to extend these findings to green growing pea plants in the phytotron at Pasadena. We did find that at the higher temperatures adenine consistently increased the area and fresh weight of the leaves but had no effect on survival, or on the stem length or total dry-weight deposition. Adenine did *not* cure the entire plant of the symptoms of high-temperature injury.

There does, of course, exist a distinct possibility, fortified by the data of Highkin, as cited by Kurtz, that in more properly conducted experiments the adenine effects would show up more dramatically. For instance, we gave the high-temperature treatment only during the dark period, on the supposition that pea growth followed the general pattern of nocturnal growth, as outlined by Went for the tomato. However, it is now clear that the thermal effects on growth of peas could be expected to be great during the light period. We were also unable, in our experiments, to grow the plants at a high enough temperature, since the phytotron at that time had no chambers at temperatures over 30°C.

Certainly the chemical cure of climatic lesions is an attractive concept as well as a catchy phrase, and research in this area is very promising. One should also remember that adenine is only one of many substances which may prove to be of some aid in combating the thermal inactivation of growth. There are many temperature-sensitive genes in *Neurospora*, and indeed many probably exist in higher plants as well. One could reasonably expect that in particular plants specific amino acids, purines, pyrimidines, vitamins, and still other compounds would be effective in extending the temperature and perhaps geographic range of the plants.

ARTHUR W. GALSTON
Department of Botany,
Josiah Willard Gibbs Laboratory,
Yale University,
New Haven, Connecticut

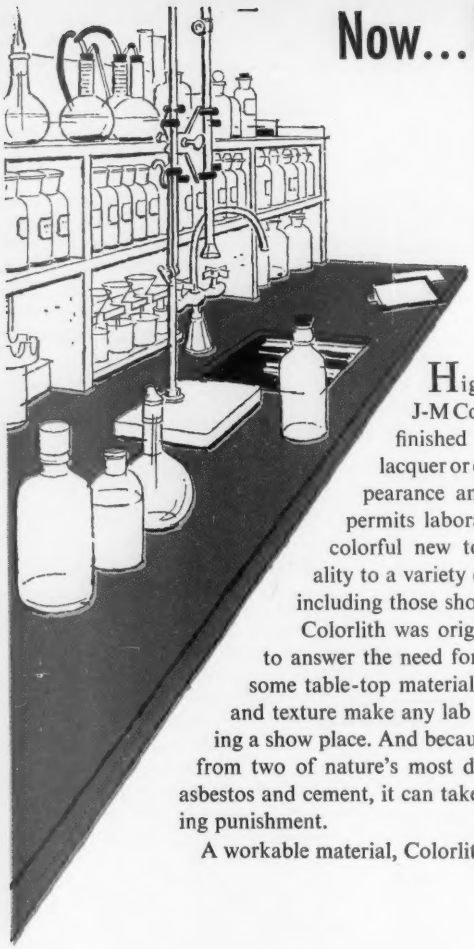
Teaching and Research

In their book *The Academic Market Place* (Basic Books, New York), two sociologists, Theodore Caplow and Reece McGee, report the results of a survey of 371 college professors and administrators. Only 4 percent believe that the test of a college teacher's ability lies in the way he teaches; about 33 percent consider the worth of a professor by the number of papers he publishes, and the remaining 63 percent give confusing answers. This survey evidently reflects a general opinion among many professors and administrators, especially in graduate schools, where the publication of papers is often considered of more importance than the clarification of ideas, which is the quintessence of teaching.

Many papers are of great value in scholarly investigations, but unfortunately some are only of use to obtain promotions for their authors. One cynic has observed that papers of this type are abstracted, quoted, and cited in a stream of other papers which in turn are abstracted, quoted, and cited. A faculty member whose chief interest is teaching often remains unknown and unsung except among a relatively limited group of students. During his academic life-time of perhaps 40 years he may reach 10,000 young people. But in a journal of international circulation a larger audience than this is obtained with the publication of only one significant paper—an audience composed of many readers of understanding and influence in the author's chosen field. This presents a temptation and one explanation of the widespread desire to become an author rather than a teacher.

To be sure, some are so gifted and energetic that they are able to publish brilliant papers, deliver inspiring lec-

(*Continued on page 403*)



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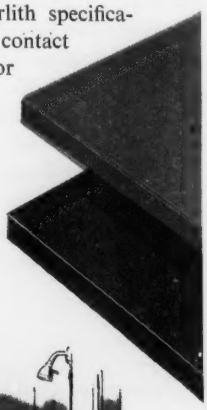

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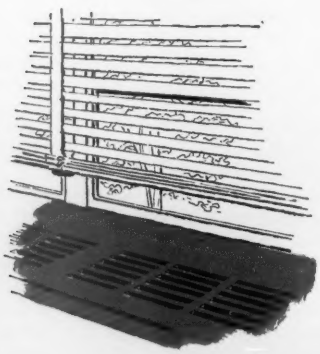

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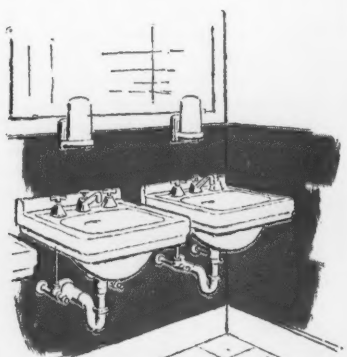

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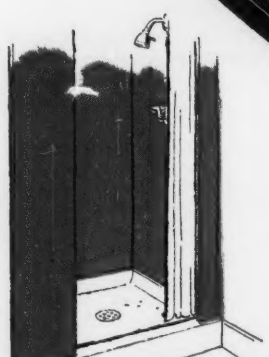

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SCIENCE, which is now combined with THE SCIENTIFIC MONTHLY, is published each Friday by the American Association for the Advancement of Science at Business Press, Lancaster, Pa. The joint journal is published in the SCIENCE format. Entered at the Lancaster, Pa., Post Office as second class matter under the Act of 3 March 1879. SCIENCE is indexed in the *Reader's Guide to Periodical Literature.*

Editorial and personnel-placement correspondence should be addressed to SCIENCE, 1515 Massachusetts Ave., NW, Washington 5, D.C. Manuscripts should be typed with double spacing and submitted in duplicate. The AAAS assumes no responsibility for the safety of manuscripts or for the opinions expressed by contributors. For detailed suggestions on the preparation of manuscripts, book reviews, and illustrations, see *Science* 125, 16 (4 Jan. 1957).

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A National Curriculum

Events since World War II have led to a searching reexamination and reappraisal of our schools. The great debate has centered around the public schools and especially around the high schools, which have been roundly, even intemperately, condemned by some and staunchly defended by others.

Those who launch the criticisms—in this as in any other discussion—attract the most public attention. We need only mention the outspoken attacks by Albert Lynd in his *Quackery in the Public Schools,* Arthur Bestor in his *Restoration of Learning,* and Admiral H. G. Rickover in his numerous speeches and in his recent book, *Education and Freedom.* The archdemon responsible for our educational ills is, according to the critical refrain, John Dewey, who has a supporting cast of lesser demons, the professional educators.

But the indictment is too simply drawn. The role of the high schools has changed markedly in the last half century. At the turn of the century, the high schools were primarily college preparatory schools, which offered academic or "solid" subjects almost exclusively. Public pressure for more educational opportunity for all, as well as other factors, led to the introduction of compulsory attendance laws. The percentage of those of high-school age in school climbed rapidly, from 11 percent in 1900 to more than 80 percent today.

The ideal has been to educate everyone to the limit of his ability and to give those headed either for a profession or for a trade a common educational experience. This common educational experience has been widely held to be essential to the maintenance of our form of democracy. According to this view, it would be a mistake to segregate, as Rickover suggests, the academically talented in special "demonstration schools." The opposing contention is that the comprehensive high school meets the needs of both those who plan to go on to college and those who plan to terminate their education with high-school graduation. With proper counseling and ability grouping in a sufficiently large high school, the needs of both groups can be adequately met, or so goes the argument. This position is strongly supported by James B. Conant in his latest book, *The American High School Today* (reviewed on page 382). Conant thinks that the best of the comprehensive schools are satisfactory and that our educational salvation lies in creating more schools equal to the best by consolidating small high schools. Only a large school can afford to be both good and comprehensive.

The increased public interest in education is a hopeful development, but it entails a potential hazard in that popular pressures may force curricular changes too hastily. A group of educators and citizens, which recently met at Stanford University under the auspices of the Ford Foundation [*Science* **129**, 316 (6 Feb. 1959)] concluded that the hazard would be reduced if a national curriculum were established. The group recommended that a nongovernmental and broadly representative commission be appointed to plan a curriculum. Such a national curriculum would establish standards by which local schools could judge their own performance. If it is granted that a commission should be appointed, the question remains who should the commissioners be? Which voices from the Tower of Babel should be amplified?—G.DuS.

13 February 1959, Volume 129, Number 3346 SCIENCE

CURRENT PROBLEMS IN RESEARCH

Shape of the Nucleus

It varies widely, from spherical for doubly magic nuclei to ellipsoidal and sometimes pear shapes.

Lawrence Wilets

Less than 50 years ago Lord Rutherford (*1*) laid the foundations for both the modern atomic and nuclear sciences with a series of celebrated experiments on the scattering of energetic alpha particles by thin foils. He used naturally radioactive materials as a source of the alpha projectiles and detected the scattered particles by visually observing flashes produced on a scintillation screen. From the pattern of light flashes, he was able to deduce that the (electrically neutral) atom consists of a positively charged, massive, nuclear core surrounded by a cloud of negatively charged electrons.

Earlier atomic theories fell; notable was J. J. Thompson's model of electrons embedded in a positively charged fluid. Two years later, in 1913, Niels Bohr (*2*) proposed his famous atomic model consisting of electrons circling the nucleus in quantized orbits.

Rutherford's experiments also yielded the first quantitative information on the size of the nucleus. The results indicated that the interaction between alpha particles and nuclei follows the Coulomb inverse square law for point charges down to distances of less than 10^{-11} centimeter. Later experiments by Rutherford and others revealed deviations from the Coulomb law, indicating structure at radii of the order of 10^{-12} centimeter.

During the past half century a diversity of experiments have been designed to measure the nuclear size and shape. These have included bombarding the nucleus with a variety of charged and neutral subatomic particles, probing the nuclear interior with its own atomic electrons, and observing nuclear radiations. Theoretical models have attempted to correlate less obvious nuclear properties, such as binding energy and reaction rates, with nuclear size.

The various experiments prior to 1953 were in qualitative agreement with respect to nuclear size, but did differ considerably when subjected to quantitative comparison. It was realized that at least part of the discrepancies could be attributed to insufficient knowledge of the nature of the probes, and another part to a lack of detailed theoretical understanding of nuclear structure.

Within the last 5 years there have been prodigious experimental advances in the detailed measurement of nuclear size, and theoreticians have been pressed to bring about a consistent interpretation of apparent disparities. The picture which is emerging is encouraging.

Within the same 5-year period exciting developments have occurred in the field of nuclear shape. Here, however, the theorists led the advances, opening up new areas of experimental research.

A picture of the gross structure—size and shape—of the nucleus can be presented now in a manner which would not have been possible 5 years ago. There is reason to believe that this picture will not be altered drastically in the next 5 years, although we look forward to a more comprehensive understanding of finer details of nuclear structure.

At this point discretion demands a few—but only a few—words concerning the use of pictures and models. While the nucleus can be described properly only in terms of actual experiments, the introduction of models can be expedient as an aid to the development of intuition and the stimulation of ingenuity. Concrete models generally contain more detail than can be verified experimentally, even in principle. Yet there can be no objection to the use of such idealized models if they are consistent with experiment and if their relationship to experiment is constantly borne in mind. Pictures, images, and models are used shamelessly in the present discourse.

This article is intended to present an "artist's conception" of the nucleus such as one might see through an out-of-focus and impossible-to-construct microscope. As the nucleus first enters the field of view of our hypothetical microscope, it appears as a formless mass. As we begin to adjust the focus, we are able to discern structure similar to that of a droplet of liquid, but with a diffuse surface. Some nuclei pulsate, passing from one shape to another. Others have permanently elongated shapes and rotate. As the focus improves, constituent parts appear. These are seen to be neutrons and protons (nucleons); occasionally there is a fleeting glimpse (10^{-23} second) of transient pi-mesons. But this is already further than we wish to proceed here, since we will not be concerned with the details of nuclear interiors.

General Nuclear Properties

The nucleus may be regarded as composed of neutrons and protons bound together by nuclear forces, the origin of which appears to lie in their mutual interaction with pi-mesons. It is far from trivial to comment that nucleons maintain their identity inside the nucleus, since one cannot easily describe other particles which are emitted by nuclei, such as electrons, neutrinos, and so forth, as existing *within* the nucleus.

The density of nuclear matter appears to be constant from one nucleus to another. To the extent that we can ascribe

Dr. Wilets, formerly of the Los Alamos Scientific Laboratory, Los Alamos, N.M., is now on the staff of the physics department, University of Washington, Seattle.

a boundary to the nucleus, this means that the nuclear volume is proportional to the nuclear mass number A. (The nuclear mass, proton, and neutron numbers are designated by the integers A, z, and N, respectively. $Z+N=A$.) For a spherical nucleus of volume $(4/3)\pi R^3$, the radius R can be written in the form

$$R = r_0 A^{1/3} \qquad (1)$$

where r_0 is a constant.

Both experimentally and theoretically, it is convenient to divide the discussion of size and shape into two general categories, according to the nature of the probe.

The first category is the matter distribution, which corresponds to the distribution of neutrons and protons. What can be measured quite accurately is the distribution of charge, which, properly speaking, is not the same as the distribution of protons, since the proton itself has a measurable charge distribution. By examining the nuclear electrostatic field, the charge distribution can be deduced; the laws of electricity and magnetism are among the best understood in physics —at least within the realm of sizes under consideration.

The second category is the nuclear force field. The force field can be measured through its interaction with other nucleons, but the interpretation with respect to matter distribution depends upon a detailed nuclear model. It is customary, therefore, to present a description of the nuclear force field as such, and then challenge the theorist for an interpretation. The problem of the nuclear force field will not be discussed here except to comment that it is similar in shape to the matter distribution, but extends about 1.5×10^{-13} centimeter further (*3*).

Radial Distribution of Nuclear Matter

Electric probes. The electron and mu-meson are excellent probes for the nuclear electric field. Both may be considered to be structureless (*4*), point particles, and both interact with nuclear matter extremely weakly (the electron, immeasurably weakly) except through the electric field. The most accurate measurements of the nuclear charge distribution come from experiments with mu-mesic atoms and high energy electron scattering.

Mu-mesic atoms. A negative mu-meson impinging on an atom can be captured by the attractive electric field of the nucleus to form a structure similar to that of the hydrogen atom. It differs from hydrogen in that the nuclear charge, Z, may be 1 to 100 times greater and that the mu-meson is 207 times as heavy as an electron. The radii of the mesic orbits are inversely proportional to both the nuclear charge and the mesic mass. The inner orbits lie well inside the atomic electron orbits, thus preserving the one-particle, hydrogen-like character of the spectrum. In heavy elements, the lowest mesic orbit is quite comparable to the nuclear radius. The mu-meson is thus amply capable of exploring the electric field of the nucleus at close range. The experimental data come from the electromagnetic radiation (in the x- and gamma-ray regions) emitted when the meson jumps from one Bohr orbit to another.

The first observation of radiation from mesic atoms was reported in 1949 by Chang (*5*), who was working with cosmic ray mesons. A theoretical discussion

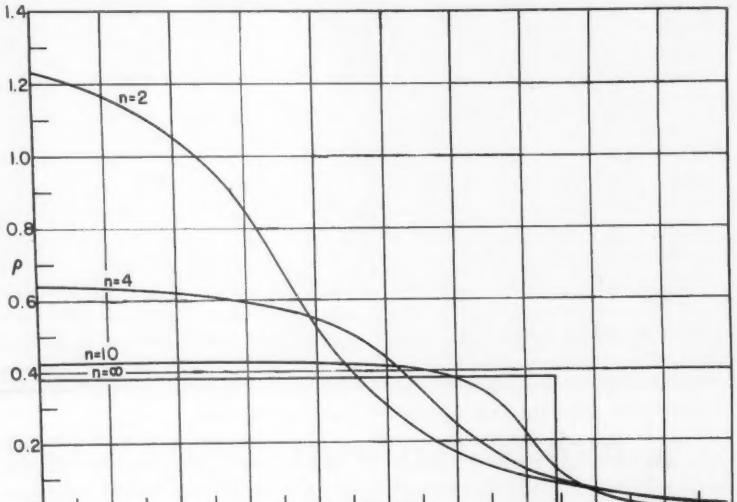

Fig. 1. A particular family of radial charge distributions, any one of which describes the mu-meson atomic radiation in lead. Each curve has only one adjustable parameter. Lengths are measured in units of the mu-meson reduced Compton wavelength, $\lambda_\mu = 2.1 \times 10^{-13}$ cm. [Reproduced from D. L. Hill and K. W. Ford, *Phys. Rev.* **94**, 1617 (1954)]

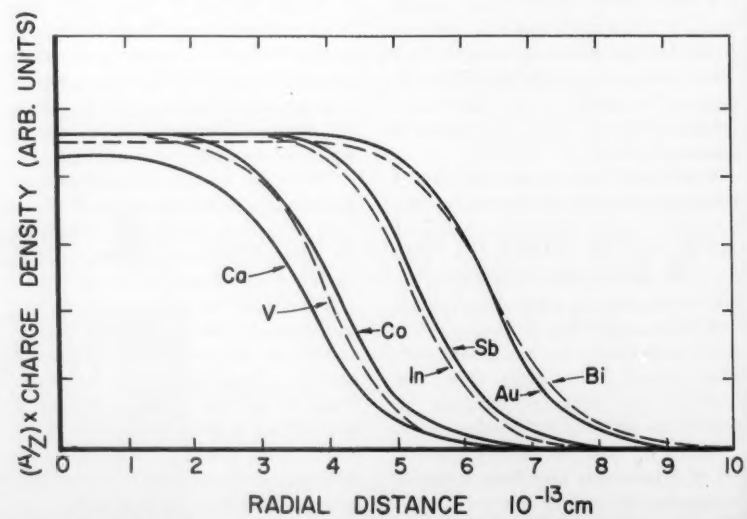

Fig. 2. Radial density (A/Z times charge) distribution curves for several elements. The curves are of the form given in Eq. 3, with two experimentally adjustable parameters, R and a. (Adapted from R. Hofstadter, *10*.)

by Wheeler (*6*) appeared the same year. With the advent of high-energy accelerators capable of producing copius numbers of mu-mesons, precision measurements on mesic atoms were begun, starting in 1953 with Fitch and Rainwater (*7*).

The present experiments have concentrated on the resonance line only, and yield a single parameter, which may be interpreted as the nuclear size (*8, 9*). There exist an infinity of different charge distributions consistent with the observed resonance line frequencies of the various elements. In Fig. 1 is shown a family of radial charge density distributions varying from exponential to uniform, all of which are compatible with experiment. Were the charge density uniform, the nuclear radius would be given by $1.17 \times 10^{13}\, A^{1/3}$ centimeter for lead. The accuracy of this number is about 1 percent, but these experiments do not yet give further information on the shape of the charge distribution.

Electron scattering. Employing the Stanford linear electron accelerator, Hofstadter and his co-workers (*10*) in 1953 began a program of electron-nuclear scattering which has contributed profoundly to our knowledge of the nuclear charge distribution. The amount of detail obtainable from the experiments is limited by the wave nature of electrons, which can only explore details of the order of, or larger than, their wavelength divided by 2π. The de Broglie relationship

$$\lambda = h/p \approx hc/E \qquad (2)$$

shows the value of using high-energy electrons (λ is the electron wavelength, h is Plank's constant, p is the electron momentum, E is the energy, and c is the velocity of light; for very energetic electrons, $E \approx cp$). At 180 Mev, the highest energy used by Hofstadter for all but the lightest nuclei, the characteristic length, $\lambda/2\pi$, is 10^{-13} centimeter. There is a limit as to how high an energy it is desirable to use, since eventually one would destroy the structure of the nucleus by the scattering process and essentially scatter independently from the individual nucleons. This would set in at wavelengths comparable to the internucleonic distance, r_0.

Analyses of the electron scattering experiments (*9–11*) are consistent with a charge distribution of rather uniform central density and a somewhat diffuse surface region. The central matter density (A/Z times the charge density) is quite constant from nucleus to nucleus. The analyses of the experiments do not rule out a central bump or depression of several percent, although neither is clearly indicated. At one time it was thought that the electrostatic repulsion between protons would tend to push charge to the nuclear surface. Because it is energetically unfavorable to separate neutrons and protons, and because nuclear matter appears to be quite incompressible (or inextensible), the theoretical indications are that a depression of more than a few percent is unlikely (*12*). At present, the electron scattering experiments are insensitive to further details in the charge distribution.

It is convenient to express the information available in terms of an explicit function for the density distribution. Reasonability criteria are applied in the selection of the expression: the function must be smooth and, from general quantum mechanical considerations, must fall off exponentially at large distances. A two-parameter function popularly chosen to express the density (in spherical symmetry) is given by

$$\rho(r) = \frac{\rho(0)}{1 + e^{(r-R)/a}} \qquad (3)$$

where $\rho(0)$ is the (approximate) central density, R is the half-density radius, and a is a surface thickness parameter.

The half-density radius for heavy nuclei follows an $A^{1/3}$ law of the form given in Eq. 1, with the empirical value of the constant given by

$$r_0 = 1.07 \times 10^{-13}\ \text{cm} \qquad (4)$$

This number can be measured to an accuracy of 1 percent for any given nucleus and fluctuates by about 2 percent from nucleus to nucleus. The value of r_0 is actually larger for lighter nuclei, since it is the central density which remains constant.

The surface thickness is frequently expressed in terms of the distance, D, in which the density falls from 90 to 10 percent of the central density. This quantity is related to the surface thickness parameter a (Eq. 3) by $D = 4.39a$. Analyses of the experiments yield (to an accuracy of about 10 percent)

$$D = 2.4 \times 10^{-13}\ \text{cm} \qquad (5)$$

independently of A. For a heavy nucleus, this is roughly one-third of the nuclear radius.

In Fig. 2 are shown experimentally ad-

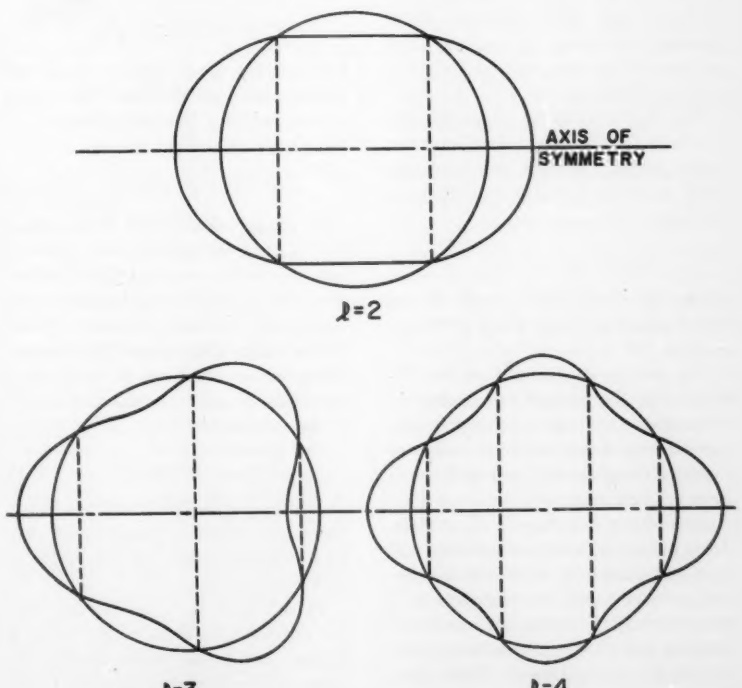

Fig. 3. Examples of the first few deformation modes. There are $(2l+1)$ modes for a given order l, but only one of each order is shown, namely, the axially symmetric mode. The order $l=0$ corresponds to a spherical shape (no deformation), while a small admixture of $l=1$ corresponds to a translation of the body without deformation. The broken curves are nodal lines, the intersection of the surface with the undeformed sphere. The number of nodal lines equals the order l.

justed distribution functions of the form of Eq. 3 for several nuclei.

Structure of the proton. The lightest nucleus, the proton, has a complex structure. Although the theory of this structure is far from complete, the following picture is helpful in understanding what is going on. One begins with the concept of a *bare* nucleon (neutron or proton), a structureless particle which constantly emits and reabsorbs pi-mesons and, to a lesser extent, other particles. The *physical* nucleon consists of the bare nucleon plus its meson cloud. A particular emission and reabsorption process which contributes to the proton charge distribution is, conceptually, of the type

$$P \rightleftarrows N + \pi^+ \qquad (6)$$

where P and N represent the bare proton and neutron, and π^+ a positively charged pi-meson. While the system is in the state given by the left-hand side of Eq. 6, the charge may be considered concentrated at a point, but when the system is in the state specified by the right-hand side of Eq. 6, the charge is distributed over a reduced meson Compton wavelength, $h/2\pi m_\pi c = 1.4 \times 10^{-13}$ centimeter, where m_π is the mass of the pi-meson. Other processes, involving strange particles, nucleons-antinucleons, and so forth, can also contribute.

The Stanford electron scattering experiments (*3, 10*) have yielded structure in the proton. The data have been analyzed, assuming a charge distribution of the form

$$\rho_{\text{prot}}(r) = e^{-r/b}/8\pi b^3 \qquad (7)$$

where the characteristic length b was found to have the value 0.23×10^{-13} centimeter.

The neutron appears to have very little charge distribution surrounding it. Even though the total net charge is zero, one can conceive of shell-like clouds of opposite charge at different radii; such have not been observed.

Distribution of protons in the nucleus. From the nuclear charge distribution and from the charge distribution of the proton, we can unfold the distribution of the *centers of the protons.* It is easier to consider first that the distribution of proton-centers, $\rho_0(\mathbf{r})$, is known. Then, given the proton charge distribution $\rho_{\text{prot}}(r)$, the charge distribution is given by

$$\rho(\mathbf{r}) = \int \rho_{\text{prot}}(|\mathbf{r} - \mathbf{r}'|)\rho_0(\mathbf{r}')\,d\tau' \qquad (8)$$

where $d\tau'$ is the volume element. Although this may be a complicated function in general, some simple features emerge directly. Consider the mean square radii, defined by

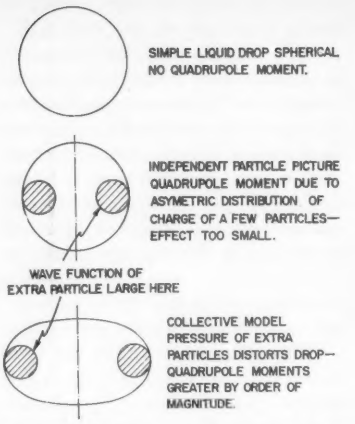

Fig. 4. An illustration of Rainwater's principle for an individual nucleon deforming a nuclear core. The shaded area represents the nucleon's wave function (orbit). Centrifugal force exerted by the nucleon deforms the core into an oblate spheroidal shape, as a marble orbiting rapidly inside a balloon would do. (Reproduced from D. L. Hill and J. A. Wheeler, *Phys. Rev.* **89**, 1106 [1953].)

$$\langle r^2 \rangle \equiv \int \rho(r) r^2 d\tau \qquad (9)$$

and similarly for the proton charge and proton-center distributions. The mean square radii for the three distributions are simply related by

$$\langle r^2 \rangle = \langle r^2 \rangle_0 + \langle r^2 \rangle_{\text{prot}} \qquad (10)$$

The charge distribution thus extends further than the proton-center distribution. For a large nucleus, the half-density radii of both distributions are nearly equal, but the surface thickness is greater for the charge distribution. The observed charge surface thickness, D, is related to that of the proton center distribution, D_0, by the relationship

$$D \approx D_0 + 4b^2/a \qquad (11)$$

with a defined in Eq. 3 and b in Eq. 7. The surface thickness for the proton-center distribution comes out to be $D_0 = 2.0 \times 10^{-13}$ centimeter.

There is little more one can say, from the experimental data, to pin down a proton matter—or mass—distribution. There is certainly mass associated with the charged cloud surrounding the proton. The pi-mesons (which probably predominate in the cloud) are lighter by a factor of nearly seven, which would tend to indicate that most of the mass is concentrated at the proton-center. But there are unresolved theoretical problems which prevent us from pursuing the problem further at this date: for example, the *nature* of the nucleonic mass is not properly understood.

Distribution of neutrons. There exist no experiments capable of yielding so "clean" a description of the neutron distribution as exist for protons. However, it is possible to obtain a measure of the difference between neutron and proton distributions. For example, at certain energies, negative pi-mesons interact more strongly with protons than do positive pi-mesons; at the same energy, the reverse is true for meson-neutron interaction. Loosely speaking, scattering with negative pis measures a proton distribution, and scattering with positive pis, a neutron distribution. Because of the nature of the pi-meson as a probe, the proton distribution so obtained will not be the same as either the charge or proton-center distributions. However, since the meson probe probably acts similarly for both neutrons and protons, the difference between (say) their half-distance radii is measured by the experiments. The best experiments to date show very little difference between the distributions (*13*):

$$R_P - R_N = (0.3 \pm 0.3) \times 10^{-13} \text{ cm} \qquad (12)$$

The smallness of this difference is contrary to earlier nuclear models which would put the protons in a spherical shell outside the neutrons—held out by their electrostatic repulsion. The explanation appears to be in a combination of factors (*12*): (i) the electric forces are weaker than nuclear forces; (ii) it is energetically favorable, because of the Pauli exclusion principle, for neutrons and protons to overlap as much as possible; and (iii) the electrostatic force under certain circumstances can act as a retaining wall to confine the protons, rather than as a dispersing influence.

Deformed Nuclei

A great deal of evidence has recently accumulated indicating that many nuclei possess shapes which differ considerably from spherical symmetry. The role of deformations in nuclear theory has been propounded by the protagonists of the so-called "collective" and "unified" models. The names associated with the theories include N. Bohr, J. Rainwater, A. Bohr, B. Mottelson, J. A. Wheeler, and others. The extent to which theory successfully charted the course of advance in this field of nuclear research is remark-

able. It is a tribute to the dynamism of A. Bohr and Mottelson and the Copenhagen school of the unified model.

In the spirit of the historical development, the present discussion begins with a description of the models, and later deals with some experimental verification. Consistent with the previous discussion of density distributions, we are still concerned here with only gross (collective) features of nuclear shape, and generally avoid discussion of internal structure.

The collective model (*14*). For the discussion of certain properties, the nucleus may be regarded as a uniformly charged, incompressible, viscosity-free, liquid drop. The dynamics of the drop are governed by the assumption of irrotational fluid flow (no vortices). It is usually assumed that a well-defined surface exists; in the case of a diffuse edge, the surface may be specifically defined as the locus of half-density points.

The only forces, (or potential energies) involved in this model are electrostatic force and surface tension (surface energy = $S \times$ area of the surface, where S is a constant. The electrostatic force tends to deform or rupture the droplet, while the surface tension tends to contain the droplet and maintain sphericity.

This is certainly a simple model. Yet with the elementary assumptions stated above, N. Bohr and J. A. Wheeler (*15*) in 1939 were able to arrive at a qualitative understanding of nuclear fission. The single parameter characterizing a nucleus

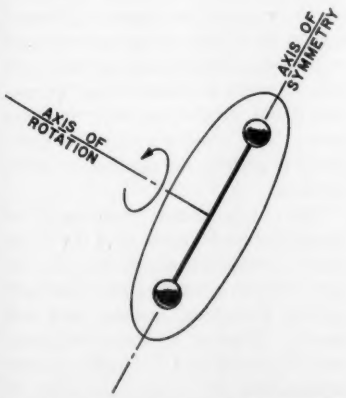

Fig. 5. Rotation of a nucleus, in analogy with a rotating dumbbell (or diatomic molecule). Because the moment of inertia is very small along the axis of symmetry, the object rotates about an axis perpendicular to the symmetry axis. The example is for an even-Z, even-N nucleus, where there is no internal angular momentum to be considered.

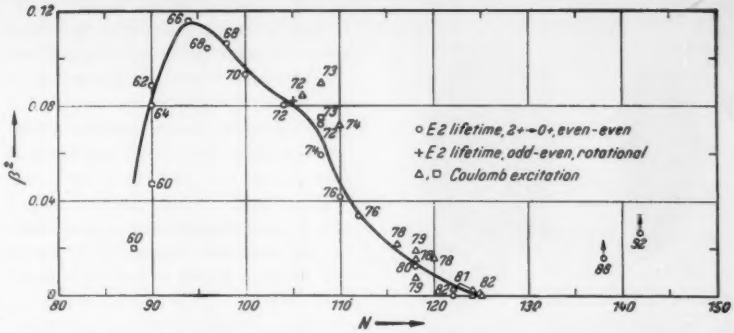

Fig. 6. The square of nuclear deformation plotted as a function of N in the first rare earth period ($\varepsilon^2 = (45/16\pi)\beta^2 = 0.895\beta^2$). Neutron magic numbers occur at 82 and 126. The proton number, Z, is given near each point. (Reproduced from K. W. Ford and D. L. Hill, 9.)

for fissionability (in this model) is the ratio, x, of electrostatic to twice surface energy:

$$x = E_{es}/2E_s = \frac{3}{5}\frac{Z^2e^2}{R}/2S\,4\pi R^2 = \frac{Z^2}{A}\Big/\left(\frac{Z^2}{A}\right)_c \qquad (13)$$

The constant $(Z^2/A)_c$ has a numerical value of 47.8. If x is greater than unity, the spherical shape is energetically unstable and fission will proceed spontaneously. But for x less than unity, the spherical shape is at least locally stable, although certain critical, deformed shapes exist beyond which further deformation, and eventually fission, is energetically favored. It requires energy of excitation to bring the droplet to these critical shapes.

For $x < 1$, and with less energy of excitation available than is required for fission, the droplet executes volume-preserving, oscillatory motion which has the nature of surface waves. If the amplitudes of oscillation are small, a normal mode analysis can be made. The modes can be classified according to the number (order l) of oscillations on the surface (see Fig. 3). (The classification can be made more precise by introducing nodal lines, defined as the intersection of the surface with the equilibrium sphere. Then the order is the number of nodal lines.) There are $(2l+1)$ independent normal modes of a given order l. The quantum mechanical energy level spectrum of the droplet is that of a set of uncoupled, harmonic oscillators.

It does not make much sense, from the viewpoint of a collective description, to include orders where the wavelength divided by 2π is comparable to, or smaller than, the mean internucleonic distance r_0. The mean wavelength is of the order of $2\pi R/l = 2\pi\, r_0 A^{1/3}/l$. This restricts l to values less than $A^{1/3}$.

The unified model (*14, 16*). A model which appears quite different from the collective model but which also had considerable success is the independent particle, or shell, model. In this model the nucleons move about freely within the nuclear interior, interacting only weakly with one another but experiencing a common nuclear potential. The unified model obtains its name from attempts to bring about a consistent interpretation of the collective and independent particle models. It was first suggested by Rainwater that individual nucleons may affect the collective behavior of the droplet. One of the most dramatic manifestations of the collective-individual nucleon interplay is the occurrence of large, permanent deformations. This is illustrated in Fig. 4. We begin with a spherical droplet, sometimes called a core. An additional nucleon moves freely within the core, the droplet surface acting to confine the nucleon. The nucleon, moving in a circular orbit as shown, exerts a centrifugal force on the droplet surface, tending to deform it into an oblate (pancake-shaped) spheroid. (A spheroid is an ellipsoid with two axes equal.) A second nucleon can fill a similar, but oppositely rotating, orbit. This will tend to double the magnitude of the deformation. But, according to the Pauli exclusion principle, only one nucleon can occupy a given orbit, and so subsequent nucleons will occupy orbits oriented less favorably to increase deformation.

Now, the nucleonic magic numbers (2, 8, 20, 28, 50, 82, 126) represent highly stable configurations for either neutrons or protons, similar to the electronic configurations in the noble gases.

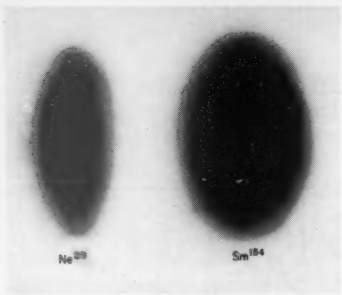

Fig. 7. Scaled "sketches" of the most deformed nucleus in the light and in the medium weight regions. Ne^{20} may have an ε as great as 0.85 (*19*), while Sm^{154} has $\varepsilon \sim 0.35$.

They are assumed to prefer spherical symmetry; when both neutron and proton configurations are magic, they form the "core" referred to in the previous paragraph. When sufficient nucleons have been added to a core to arrive at the next higher doubly magic number, the nuclear equilibrium shape returns to spherical. It is interesting to note that one particle less than a magic number (sometimes referred to as a hole) exerts a negative pressure on the surface, leading to a prolate (cigar-shaped) spheroid. Midway between magic numbers, the simple theory predicts a switch from oblate to prolate. Details in the nuclear force, however, lead to a predominance of prolate shapes. We shall refer below primarily to prolate shapes, although nearly everything is equally valid for the oblate case.

It is clear that the magnitude of nuclear deformations depends on the details of the internal structure of the nucleus, and may be expected to vary considerably, from nucleus to nucleus. The variation has some regularity, nevertheless, the deformations reaching a maximum between magic configurations.

In addition to being able to vibrate about the deformed equilibrium shape, the spheroidal droplet can also rotate. In fact, it is energetically easier (in the sense that the quantum mechanical energy levels lie lower) for the spheroid to rotate than to vibrate.

If we look for a macroscopic analog of our cigar-shaped nucleus, we should not look to a rigid body of this shape. An egg might be better, so long as it is not hard-boiled (*17*). The point is this: of the three moments of inertia for a rotating nucleus, the moment for rotations about the symmetry axis (axis of revolution) vanishes. A body nearly having such a property is a dumbbell (or diatomic molecule) in which the weights lie very close to the rod (see Fig. 5). No matter how an *idealized* dumbbell is thrown into the air, it always rotates end over end, about an axis perpendicular to to the rod.

The smallness of the moment of inertia about the symmetry axis is an interesting and important point. It has been given considerable theoretical attention, along with the entire nuclear moment of inertia problem. In spite of sophisticated progress, an early naïve analogy still serves as the best aid to understanding. Consider a bucket of water which is maintained upright and spun about its vertical, central axis. If there were no viscosity between the bucket and the water, the water would not be set in motion and hence would offer no inertial resistance to the rotation. Consider next the bucket to be bent so that from above it looks like an ellipse; rotation about the same axis now necessitates moving water. As the deformation of the bucket is increased, the moment of inertia associated with the water increases. The rotation of the circular bucket corresponds to droplet rotations about the symmetry axis, while rotation of the distorted, elliptical bucket corresponds to droplet rotation about an axis perpendicular to the symmetry axis.

Consequences of nuclear deformations (*16*). One rather direct manifestation of nuclear deformations is the deviation of the nuclear electric field from spherical symmetry. A quantity of interest is the nuclear quadrupole moment, defined by

$$Q = \int \rho(\mathbf{r})\,(3z^2 - r^2)\,d\tau \qquad (14)$$

where $\rho(\mathbf{r})$ is the nuclear electric charge density and $d\tau$ is the elemental volume. (Note that Q vanishes if $\rho(\mathbf{r})$ is spherically symmetric.)

Static quadrupole moments can be measured by atomic spectroscopy and molecular resonance techniques. Furthermore, a rotating or oscillating quadrupole can radiate or absorb electromagnetic energy. The rate of emission (lifetime) or probability for absorption (cross section) of radiation gives a measure of the *transition* quadrupole moment. Although these two moments are operationally different quantities, the model provides a relationship between them in terms of an *intrinsic* quadrupole moment. For a uniformly charged spheroid, the intrinsic quadrupole moment, Q_0 (defined as in Eq. 14, with z taken along the nuclear symmetry axis), is related to the deformation by

$$Q_0 \cong \frac{4}{5} ZR^2\varepsilon \qquad (15)$$

where R is the mean nuclear radius, and the deformation parameter, ε, is defined here as the difference between the length of the spheroid along the symmetry axis and the width perpendicular to the symmetry axis, divided by twice the mean radius (*18*). Positive values of ε correspond to prolate, and negative to oblate, spheroids.

Other experimental verifications of the unified model come from the occurrence of rotational energy level spectra, new selection rules on alpha, beta, and gamma transitions, nuclear spins and parities, details of level structure, magnetic moments, and a variety of other experiments.

Occurrence of nuclear deformations. Large permanent deformations of the type discussed above occur with great regularity in three regions of the nuclear masses. These are $19 < A < 25$, $150 < A < 185$, and $220 < A$. (Note that doubly magic nuclei appear at $A = 8 + 8 = 16$, $50 + 82 = 132$, and $82 + 126 = 208$.) The

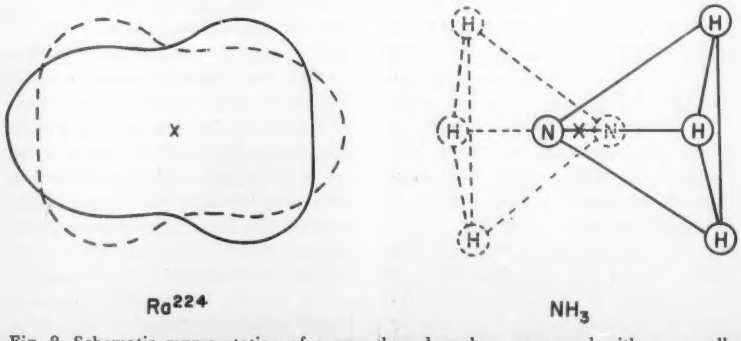

Fig. 8. Schematic representation of a pear-shaped nucleus, compared with an equally schematic representation of the ammonia molecule, NH_3. The broken curves indicate the inverted configurations. The crosses are the centers of mass.

lightest region was the most recently recognized (*19*), coming as a bit of surprise, since collective concepts were not expected to be valid for so few nucleons. The other two regions correspond coincidently to the chemical rare earth regions. Large deformations actually appear very abruptly at $N = 90$ (near A of 150) and depend more strongly on N than on Z in the first rare earth region. Similarly, large deformations begin again at $Z = 88$ (near A of 220) and depend more strongly on Z in the second rare earth region. All nuclei so far observed in the limits of these last two regions display unified model characteristics.

In Fig. 6 is shown a plot of experimentally determined deformations in the first rare earth region. Note the sharp rise at 90 neutrons, and the smallness of the deformations at the doubly magic numbers. In Fig. 7 are scaled drawings of the most deformed nuclei in the light and intermediate mass regions, namely, Ne^{20} and Sm^{154}.

Pear-shaped nuclei. The final type of deformation to be discussed is the "pear-shape." This is a deformation involving a combination of both $l=2$ and $l=3$ modes (see Fig. 8).

Speculation that such pear-shapes exist was first presented by Christy (*20*), and is based on details of the rotational-vibrational energy level spectrum. Certain isotopes in the neighborhood of radium exhibit a characteristic spectrum analogous to the so-called inversion spectrum of the ammonia molecule, NH_3. Both spectra have been interpreted as corresponding to oscillations of the objects between one relatively stable form to its mirror image, which is equally stable. This is illustrated in Fig. 8; the "mirror" passes through the center of masses of the objects and is oriented normal to the axes of symmetry.

The asymmetrical shape of such nuclei may also have relevance to the problem of mass asymmetry in fission. Most nuclei fission into fragments of quite unequal masses, which indicates that at some point before splitting actually occurs, fissioning nuclei prefer an asymmetrical shape.

Summary

The gross features of nuclear morphology can be summarized as follows. Nuclei have shapes similar to a diffuse-surfaced, liquid drop. The interior density is rather uniform, and also constant from nucleus to nucleus. The constancy of nuclear density implies an $A^{1/3}$ law for the mean nuclear radius, the proportionality constant being 1.07×10^{-13} centimeter. The surface region is diffuse, the nuclear density falling from 90 to 10 percent of the central value in a distance of about 2.4×10^{-13} centimeter, independently of nuclear mass number.

Nuclear shapes can vary rather widely, with doubly magic nuclei preferring spherical symmetry. Some nuclei execute volume-preserving oscillations about spherical shape, while others possess permanent spheroidal deformations. The values of the deformation parameter, ε, for such spheroids possibly attains 0.85 for some light nuclei and 0.4 for some intermediate weight nuclei.

There is some evidence, based on the occurrence of "inversion" spectra and asymmetrical fission, that pear-shaped nuclei may also exist.

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16. J. Rainwater, *Phys. Rev.* **79**, 432 (1950); A. Bohr and B. R. Mottelson, *Kgl. Danske Videnskab. Selskab. Mat.-fys. Medd.* **27**, No. 16 (1953). A general review is given by S. Moszkowski in *Encyclopedia of Physics* (Springer, Heidelberg, 1957), vol. 39, p. 411.
17. A hard-boiled egg is essentially a solid body with three nonvanishing moments of inertia. A raw egg nearly satisfies the idealized bucket experiment of the next paragraph of this article. Rotation of the egg shell about its symmetry axis is decoupled from the egg fluid so there is very little moment of inertia about this axis. One can easily verify the difficulty of spinning a raw egg about its symmetry axis. If one gives a spin and lets go, the egg usually ends up spinning about an axis normal to the symmetry axis.
18. The deformation parameter ε defined here is related to Bohr's β (*14*) by $\varepsilon = (45/16\pi)^{1/2}\beta = 0.946\beta$.
19. See, for example, G. Rakavy, *Nuclear Phys.* **4**, 375 (1957).
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Basic Research—Description versus Definition

A definition of basic research in probability terms is useful, but statistics based thereon are not.

Charles V. Kidd

"An abstract term is like a box with a false bottom: you may put in it what ideas you please, and take them out again without being observed."—ALEXIS DE TOCQUEVILLE (*1*).

Descriptions and definitions of basic research have at least two kinds of potential uses. The first is to convey—generally to nonscientists—a sense of the nature of basic research, a feeling for its importance, and an appreciation of the motives and working conditions of scientists. One ultimate purpose served by such a description is to expand the scientific capacity of the country by creating understanding of, sympathy for, and support for, the full array of conditions that seem to be conducive to the production of basic findings.

To serve this function satisfactorily, basic research can be described in general, impressionistic terms, and logical precision is not required.

Vannevar Bush, among others, has written such an impressionistic description (*2*):

"Basic research results in general knowledge and an understanding of nature by its laws. This general knowledge provides the means of answering a large number of practical problems. The scientist doing basic research may not be at all interested in the practical applications of his work yet the further progress of industrial development would eventually stagnate if basic research were long neglected. New products and new processes do not appear full grown. They are founded on new principles and new conceptions, which in turn are painstakingly developed by research in the purest realms of science. A nation which depends upon others for its new basic knowledge will be slow in industrial progress and weak in its cooperative position in world trade, regardless of its mechanical skill."

A second use of definitions of basic research is to provide rational, and adequately precise, criteria for decisions required in classifying research as basic for the purpose of compiling statistics.

The burden of this article is that basic research can be and has been described adequately for the first use, but that basic research has not yet been defined—and may never be defined—so as to permit an unambiguous, objective measurement of the dollars spent for basic research in this country.

Problem of Definition

The fact that the problem of securing an adequate definition of basic research has not been resolved is made clear in a recent report of the National Science Foundation (*3*):

"University officials estimate that, during the academic year 1953–54, academic departments of colleges and universities and agricultural experiment stations received about $85 million for basic research from the Federal government. But Federal officials estimate that they provided barely half that amount to universities for the same purpose and during the same period."

Somewhere between the offices in Washington which hand out research funds and answer questionnaires and the offices in universities which receive funds and answer questionnaires, the meaning of the definitions of basic research undergoes a metamorphosis that permits one set of observers to find the quantity to be twice as large as the other observers say it is. Such a discrepancy raises a number of questions, including the nature of the definitions that provide such a flexible yardstick.

Investigator-Centered Definitions

A useful point of departure is the definition of basic research given to both federal agencies and universities by the National Science Foundation as a guide to classification of research (*4*):

"Basic research is that type of research directed towards increase of knowledge in science. It is research where the primary aim of the investigator is a fuller knowledge or understanding of the subject under study, rather than a practical application thereof."

The salient characteristic of this definition, it seems to me, is that it is framed in terms of the "aim," or the intent, or the motive of the investigator and not in terms of the research finding itself. This thought led me to collect definitions of basic research and to try to group them in various ways.

Without pretending to have exhausted the subject, I have found that definitions of basic research seem to fall into two general categories. There are first those, such as the National Science Foundation definition cited above, which define research in terms of investigators' motives and intent and the conditions under which they work. The second group of definitions relates not to investigators but to the work itself.

Let us look first at some of the definitions that are investigator-centered. A historian of science, I. B. Cohen, has spelled out a definition in these terms (*5*):

"The difference between those who work at fundamental research and those who work at applied research is in the point of view with which they face the problem and the goals they have in mind. The man working at the 'pure science' end of the spectrum, whether in a university or in an industrial laboratory, pursues a problem because it is interesting or because it appears to have a certain relevance to fundamental knowledge. By contrast, the man working at the applied science end of the spectrum

The author is chief of the Office of Research Planning of the National Institutes of Health, Bethesda, Md. This article is taken from a forthcoming book, "Universities and Federal Research" (Harvard University Press).

pursues a problem because it has a relevance to a particular practical goal."

As another example, A. M. Brues has stated in the *Bulletin of the Atomic Scientists* (*6*):

"Basic research, now, is an attitude of curiosity about underlying relations between things, and about fundamentals; it can be pursued either by abstraction or through ability to follow up unexpected findings."

J. A. Stratton, former provost of Massachusetts Institute of Technology, has stated the motive or intent criterion in this way (*7*):

"Research in the natural sciences before the 17th and 18th centuries began largely as an avocation of amateurs, of gentlemen of leisure with a curiosity about the nature of the physical world. It was *fundamental* in that it was motivated wholly by a desire to know and understand."

Conant has taken the same approach (*8*):

"One may consider science as an attempt to either lower the degree of empiricism or to extend the range of theory. . . . Almost all significant work of scientists today, I believe, comes under the heading of attempts to reduce the degree of empiricism; the distinction between one group and another is in the motivation. Those who are interested in the fabric of science as such are ready to follow any lead that gives promise of being fruitful in terms of extending theoretical knowledge."

A distinct subgroup of investigator-centered definitions distinguishes between basic and applied research in terms of conditions under which the investigator works, and particularly of the degree of freedom he enjoys. Thus, basic research is sometimes defined as research undertaken by the "uncommitted" investigator—that is, an investigator who is not bound by external forces to follow a predetermined line of study. Such a definition appears in *Basic Research, a National Resource,* a 1957 publication of the National Science Foundation (*3*):

"Basic research is systematic, but without direction save that which the investigator himself gives it to meet the challenge of the unknown. He is strictly on his own, guided primarily by his interest in learning more about the workings of nature."

There is a logical distinction between the motive and intent of investigators on the one hand and the degree of freedom with which they work on the other hand. However, the two are often considered together, as is the case in the quotation immediately above. In any event, both criteria are clearly centered around the scientist—his motives and intent and the degree of freedom with which he works.

Substance-Centered Definitions

Let us turn now to definitions of basic research which center around the substance of research.

One category within this group distinguishes between basic and applied research in terms of the prospective utility of findings in meeting some practical need in the near future. Here the criterion is that the work, as described in advance, does not appear to have any immediate practical application. Such a definition, intended to indicate the kind of basic research the National Science Foundation and other federal agencies should support, appears in Executive Order 10521 (21 March 1954): ". . . support by other Federal agencies of basic research in areas which are closely related to their missions is encouraged." Such research is designated as "special purpose" basic research, in contrast to "general purpose" basic research to be supported by the National Science Foundation. Unfortunately, this definition in practice does not provide a usable means of distinguishing between types of research because there is no inherent distinction, particularly before the work is initiated, between the substance of "special purpose" and that of "general purpose" basic research.

In the second category of substance-centered definitions, distinction is made between basic and applied research in terms of the scientific significance of findings.

There is a wide agreement among scientists that breadth of findings is the criterion for assessing the basic character of a discovery. For example, Poincaré has stated (*9*):

"There is a hierarchy of facts. Some are without any positive bearing, and teach us nothing but themselves. There are, on the other hand, facts that give a large return, each of which teaches us a new law."

Hardy, the mathematician, has noted that (*10*) "mathematicians value ideas by their generality and depth."

Conant wrote (*11*):

"Systemized or well ordered empirical inquiries are one element in the advancement of science; the other element is the use of new concepts, new conceptual schemes that serve as working hypotheses on a grand scale. Only by the use of new ideas of broad significance has science advanced. . . ."

This criterion of the generality, breadth, or significance of findings has been well summarized by Cohen (*5*):

"We thus naturally classify scientific work according to the degree whereby it affects scientific thought and procedures; according to the amount by which it changes the foundation or structure of science itself. We may call this the fundamental character of the research. Some work is of a more fundamental character than other work simply because it affects a broader area, or because within its narrow area of applicability it has a deep and penetrating effect."

Contradictions and Inadequacies

If each investigator-centered and substance-centered definition is taken literally as satisfactory and self-contained, as is often done, each of them is patently inadequate.

For example, the definition of the National Science Foundation—that basic research is research "where the primary aim of the investigator is full knowledge or understanding of the subject under study, rather than a practical application thereof"—invites the obvious rejoinder that persons aiming to solve a very practical problem have produced findings of general significance. This definition, drawn up for the foundation's statistical reports on the volume of basic research conducted in this country and fairly widely used for other purposes, suffers from the further deficiency that decisions as to who will or who will not receive a research grant cannot in practice be based on assessment of scientists' motives. Accordingly, as the deputy director of the National Science Foundation has stated in describing how the foundation selects research proposals for support, "There is really only one criterion, and that is the excellence of the particular research proposal which is made to the Foundation" (*12*). Implicit in this procedure, which is not literally followed in practice, is a definition of basic research which relies on the substance of proposed research rather than on an assessment of the man or of his motive, intent, or working conditions.

Those who define basic research solely

as research conducted by investigators free to follow wherever their findings and curiosity lead are open to the observation that some research performed under these conditions is worthless. Furthermore, if basic research is defined solely as work with no foreseeable application, some trivial and irrelevant research will be admitted to the category of basic research.

Apart from the inadequacies of each definition, the group of investigator-centered and the group of substance-centered definitions of basic research are, if accepted literally as adequate self-contained definitions, manually exclusive. Thus, if the breadth of findings is to be the criterion for defining basic research, such things as the investigator's motive and the freedom with which he works are irrelevant. Conversely, if the attitude and approach of the investigator and the degree of freedom which he enjoys are to be the exclusive criteria defining basic research, the nature of the findings has no bearing on whether research is basic or not.

Each of the criteria, when considered singly and literally, does lead to contradictions which have been unwisely invoked to ridicule the concept of basic research. Yet if the idea of basic research is critically important, as it certainly is, it is also important that a generally acceptable definition be worked out.

Reconciliation

Inadequate and mutually exclusive definitions of basic research are currently used without apparent concern by intelligent and experienced people. Such a situation leads to the suspicion that there may be some underlying problem forestalling a clear resolution of the contradictions pointed out above. I think that there is such a problem, arising from an unapparent confusion of ends and means.

To reconcile the various definitions of basic research in a way that is not only logical but operationally useful, it seems to me imperative to begin by drawing a distinction, pointed out by others, which may appear to be overly nice. "Basic findings" are fundamentally different from "basic research" because findings are an end product and research is a process. This distinction, seen clearly by Brues (*6*), is not always drawn, even though it is of central importance to a clear resolution of the problem of definition. Failure to draw the distinction generally leads to confusion. This is illustrated by Cohen's statement quoted above. The system of classification implied by his definition is ambiguous because one cannot tell whether the *work* referred to is the process of research or the findings. Literally, this *work* is a process, but the sense of the word *work* in the context of Cohen's description is "findings."

For the administrator, definitions in terms of the end product—basic science or basic findings—are not usable because decisions must be made before the research is completed and the findings are known. Administrators are forced to be prophets. They must support basic research before the returns are in. This can actually be done with an adequate degree of precision by observing the kinds of people, the kinds of motives, and the kinds of working conditions that have as a matter of probability tended to produce basic findings.

With this view of the problem of definition, the array of criteria used to describe the research process must be viewed not as literal descriptive definitions but as statements of the probability of producing a basic finding. Thus, a "definition" of basic research in terms of the investigator's freedom is simply the statement of an assumption that those whose thought is not restricted and narrowly channeled are more likely to come forth with scientific ideas of great breadth or depth than are those working with less freedom. Definitions in terms of the motive and intent of the investigator are essentially statements of a belief that those with wide-ranging native curiosity are more likely than others to produce basic findings.

When the criteria of basic research are viewed as statements of the probability that basic findings will be produced under certain conditions, rather than as a literal description of the process of basic research itself, apparent contradictions disappear. For example, the "no practical application" criterion would mean not that no one working on an applied problem can produce a basic finding but simply that the probability of producing a fundamental finding is greater among those whose thinking is not restricted by a search for application. Definition in terms of the freedom with which the investigator works would not mean that basic findings are produced by all of those who are free to do whatever they wish but that the probability of producing new ideas of broad significance is greater among investigators who are free.

Probability Definition for Collection of Statistics

A definition of basic research in terms of the circumstances that appear as a matter of probability to lead to basic findings is inherently unsuitable for the purpose of collecting statistics. One reason for this is that a sound and usable definition of the conditions under which basic findings are, as a matter of probability, most likely to be produced must encompass all of the predisposing factors. Motives, intent, working conditions, and prospective applicability of findings must all be included. Other circumstances that appear to increase the probability of producing basic findings may be added to those already generally accepted. Just what circumstances should be included in a definition, and the weight to be given to each, are matters decided in large part by the exercise of subjective judgment.

Second, motives, attitudes, and working conditions cannot be measured precisely. How curious must a scientist be about fundamental phenomena before his work is viewed as basic research? How free must he be? And free from what, or for what? How remote from application must his findings be before his research is considered basic? These questions suggest that even if there were a firm consensus as to the criteria that are properly a part of a definition of basic research, it would be impossible to measure such factors quantitatively and comparably.

The criteria to be used in defining basic research and the weight to be given to each are both affected by such things as institutional goals, traditions, and personal experiences and predilections. This explains why, as shown in the first part of this article, people in universities have looked at a given universe of research and have decided that the proportion of this research belonging in the basic category is twice as large as the proportion placed in the basic category by federal administrators. As another example of the nature of this problem, if engineers and physicists had to classify each others' work as basic or applied, less engineering and more physics would be called basic than would be the case if each discipline classified the work that is done in its own fields.

It may be that these difficult problems of definition can be overcome in time. Magnitudes that were in earlier years the source of sharp debates are now measured by generally accepted

techniques. For example, such things as the national income accounts—the gross national product, private investment, savings, and so forth—have evolved into standard statistical series only after years of sustained effort and critical discussion by a large group of economists.

Whether a comparable effort could produce a generally accepted set of statistics dealing with basic research is a matter of judgment. For the reasons set forth above, it seems to me that the problem is inherently unsolvable, and that efforts to secure adequately precise and comparable statistics by undertaking to improve the definition of basic research are therefore futile. But I could be wrong.

Even if statistics on basic research are inherently affected by subjective judgments, it may be better to collect and publish what can be collected than to make no effort to do so. The case for making the effort rests essentially upon our strong national predilection to rely upon statistics in reaching judgments. If one accepts the idea that the nation would be better off if greater attention were paid to basic research, and if statistics help to convince people of the validity of the idea, it may be worth while collecting and publishing the information even though the statistics are inherently allegorical.

Probability Definition for Administrative Decisions

Definitions of basic research in terms of such factors as the degree of freedom with which the investigator works and the prospective applicability of his findings are useful in making administrative decisions on the support of research. In practice, administrators do not decide to support work because it is basic or not basic. Indeed, the term *basic research* is used much less frequently in the day-to-day business of research administration than it is in communicating with the nonscientific world.

Administrators consider the man—his past performance as judged by his peers—even though the merit of the research project is ostensibly the basis for judgment. They consider the facilities available to him. They take into account the support available in his field—whether it is a "gap area" or one well financed. What those who make decisions cannot do and do not attempt to do is to judge the intentions and the motives of investigators. Definitions of basic research in terms of motive and intent are, in practice, used by administrators—those who participate in decisions on the distribution of research funds—neither in administering research nor in collecting statistics on research.

The criterion of freedom of the investigator as a condition conducive to the production of basic findings is also usable and used as a guide to research administration. More broadly, this criterion encompasses the total array of factors conducive to scientific research of high quality. H. A. Shepard, in an article in the *Journal of the Philosophy of Science* (*13*), came to this conclusion:

"Efforts to define basic research operationally are misleading and bring about neglect of the forces that produce it—the training, discipline, values, way of life and system of social control that motivate men to advance knowledge for its own sake. . . . Support for basic research means support of a social system which so motivates men."

I am suggesting, first, that it is not possible to define basic research operationally. Second, I think that basic research can be effectively promoted by concentrating on provision of funds under terms and conditions designed to strengthen the forces, values, and social system which appear as a matter of probability well designed to promote basic findings.

Administrators—federal, university, or industrial—do have it within their power either to give scientists a large degree of freedom or to hem them in with a wide array of well-know requirements and restrictions. Freedom means, here, broad definition of areas of research; easy, informal changes in the direction of the research; assured stability of support; and freedom from onerous and essentially unproductive reporting requirements. In administrative terms, the terms and conditions under which all federal funds are provided affect basic research as significantly as does provision of funds for work labeled as basic. To the extent that scientific freedom affects the character of findings, a plea that the Federal Government "support more basic research" is a plea for administration of a larger proportion of federal research funds in a manner which places few restrictions upon investigators.

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Carotene-Donor-Acceptor Complexes in Photosynthesis

The predicted lowering of the excited states of carotenoids may offer a new photosynthetic pathway.

John R. Platt

It has appeared for some time that carotenoids probably play some role in photosynthesis. This is suggested by their universal occurrence as the second major pigment in all autotrophic plants, by their rate of formation parallel to that of chlorophyll in corn seedings (*1*), by the carotene-like changes in the absorption spectrum of *Chlorella* under illumination (*2, 3*), and by their known ability to quench the triplet state of chlorophyll in organic solvents (*4*).

It is not yet settled whether the carotenoids are actually part of the primary photosynthetic sequence. In some organisms they have appeared to be (*5*); and an ether-soluble fraction containing carotenoids is essential for the Hill reaction in isolated chloroplasts (*6*). But here the active component now seems to be vitamin K (*7*). And carotene deficiencies in *Chlorella* and *Chromatium* and several photosynthetic bacteria produce simply an aerobic photosensitivity, so that the cells die under air and light (*8–10*). This suggests that the carotenoids only protect the cell from photooxidation (*9*). Also, they do not appear to play any part in the photoproduction of high-energy phosphate bonds (*10*).

On the other hand, the carotene-free cells have a changed spectrum even in the chlorophyll peaks, indicating that there is normally an association between the two molecules (*2, 9, 10*). Such spectral changes alone could affect the time constants and the photosynthetic pathways through either the singlet or the triplet states of the chlorophyll.

Spectral Shifts

In any case, the evidence for some kind of carotene participation is strong enough that Strehler has asked (*11*) whether light energy might not be transferred sometimes from photoexcited chlorophyll to a carotenoid (rather than in the well-known reverse direction), with the latter then playing some essential role in "mediating" an electron-transfer from a donor to an acceptor molecule in the solution. At first this sounds energetically impossible, since the lowest excited state of chlorophyll is below that of carotene. But carotene is a conjugated chain molecule, and such molecules have the unique spectroscopic property that their spectra and excited states can often be shifted very far by solvent and polarization effects (*12, 13*). Kropf and Hubbard (*14*) have already proposed such shifts for retinene in the complex visual pigment, rhodopsin.

It is the purpose of this article to show that similar spectral shifts should occur for a particular type of carotene complex, and that Strehler's suggestion then opens up certain new possibilities for energy transfer and electron transfer in carotene-containing photosynthetic systems, whether the carotene plays a primary or a secondary role. This adds one more to the numerous theoretical pathways described in the literature (*11*), which have not yet been experimentally excluded. It is worth noting that such energy-shifts and electron-shifts in conjugated-chain complexes might also be important elsewhere in biology.

Nature of the predicted shifts. The spectral shifts depend on the fact that a conjugated chain of N atoms has a maximum wavelength of absorption only when it can be written with two extreme resonance structures that are equivalent (*12*). This "isoenergetic wavelength" is near $\lambda = N(500\ \text{A})$, or at about 11,000 A for a system as long as carotene, with a 22-atom chain (*12, 13, 15*). When the extreme resonance structures are non-equivalent, the peak is shifted (quadratically) to shorter wavelengths and higher energies, reaching its shortest position for a polyene

$$H_2C{=}CH{-}CH{=}CH{-}\ldots{-}CH{=}CH_2$$

where only one nonionic resonance structure can be written for the ground state. As a result, the carotene peak is actually at about 4800 A. [H. Kuhn showed (*15*) that this increase of energy is due physically to the alternation of single and double bond lengths, which produces a Brillouin gap when one resonance structure is dominant.]

But now suppose that an electron donor molecule, *D*, approaches one end of the polyene chain while an electron acceptor, A, approaches the other. The resulting system

$$D \cdot {=}{-}{=}{-} \ldots {-}{=}{-}{=} \cdot A$$

would be a "trimolecular charge-transfer complex," $D \cdot Car \cdot A$, like the bimolecular "charge-transfer complexes," $D \cdot A$, whose interactions and absorption spectra to a $D^+ \cdot A^-$ excited state have been described by Mulliken (*16*). By analogy with the latter case, the ground state of $D \cdot Car \cdot A$ should get an increased contribution from a second resonance structure, a stabilized ionic structure of the form

$$D^+ \cdot {}^-{=}{-}{=} \ldots {=}{-}{=}{-}^+ \cdot A^-$$

and the latter structure should dominate the first excited state. The plus and minus signs here indicate only the electron shifts and not necessarily the total charge, since the donor and acceptor molecules themselves may be neutral or ions or radicals (*16*).

Any such stabilization of the ground state would be cooperative, and the presence of either *D* or *A* would facilitate the acquisition of the other. The trimolecular arrangement would, of course, be competing with ordinary $D \cdot A$ complexes in the same system. But for any $D \cdot Car \cdot A$ complex that is formed, evidently the increased equivalence of the two resonance structures *should shift the carotene absorption peak to much longer wavelengths* (*14*), as is shown in Fig. 1. Depending on the strength of the complex, the peak might lie at any position out to the isoenergetic wavelength at about 11,000 A. [The curve shape should also change as shown (*12, 13*), but this need not concern us here.]

This situation is analogous to the converse case of a donor-acceptor (dipolar)

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molecule in a polarizable solvent, which is realized experimentally with Brooker's merocyanine dyes (*12*). These dyes are of the general type

$$O{=}CH{-}CH{=}CH{-}\ \ldots\ {-}CH{=}CH{-}NH_2.$$

They have a second, dipolar, ionic structure of the form

$$^{-}O{-}CH{=}CH{-}CH{=}\ \ldots\ {=}CH{-}CH{=}NH_2^{+}$$

and the equivalence of the two structures can be changed and the spectra shifted over a range of thousands of angstroms simply by varying the polarity of the solvent; Fig. 1 is in fact based on these spectra. It is therefore not a very great extrapolation to predict a similarly large shift for the present polyene complexes. It would be important to examine some four-component solution systems experimentally to look for such trimolecular complexes and their spectral shifts.

Some support for these predictions may be provided by the blue and green colors of adducts of carotene with Lewis acids or bases, which have been reviewed by Körösy (*17*). The products are complex and unstable mixtures, not well characterized chemically, but they seem to confirm that carotenes can serve either as electron donors or acceptors in bimolecular complexes. The colors might be due either to charge-transfer spectra of the Mulliken kind or to spectral shifts of the carotene peak as described here, but the latter should give a somewhat more limited range of spectral variance, and the comparative constancy of the colors with reagents of various types may therefore favor the latter interpretation.

Possible Photosynthetic Pathway

The part that such shifts in the $D \cdot Car \cdot A$ spectra might play in a photosynthetic mechanism is suggested in Fig. 2. This is an energy level diagram for the singlets (or the triplets) of a system containing many chlorophyll and carotene molecules, as well as donors and acceptors. The first excited states of free or weakly complexed carotenes will lie above those of chlorophyll. But if any carotene is complexed by a sufficiently strong (and sterically suited) donor and acceptor, giving $D \cdot Car \cdot A$, then its first excited singlet (or triplet) state, by our prediction, might be shifted so that it lies below the singlets (or triplets) of the chlorophylls. *In this case, the* D · Car · A *complex will be the energy sink for the excited states of the whole system.*

The light energy absorbed by any molecule in the vicinity will cascade downward by the usual radiationless transitions to the first excited states of the chlorophylls, and thence by the usual spatial energy transfers among these chlorophylls to the unique $D^+ \cdot Car \cdot A^-$ excited state. This process might apply either to the singlet or the triplet states, since, if the wavelength shifts are large enough, the $D^+ \cdot Car \cdot A^-$ triplet can also be brought below the chlorophyll triplet state. [The twisting of the equilibrium configuration of the lowest polyene singlet and triplet (*18*) is a further complication, but it takes some time to accomplish and may not be competitive with fast processes.]

It seems likely that, if its energy is adequate, such a $D^+ \cdot Car \cdot A^-$ excited state could then go easily by a radiationless transition to a separated configuration, in which the D^+ and A^- molecules (ions or radicals, or both) move away and the electrons in the polarized carotene molecule shift back to their free-carotene ground state arrangement, the carotene being a kind of photocatalyst, much as Strehler suggested. He has called it an "electron mediator," a term which emphasizes correctly its intermediate role here and the fact that it is *not* acting as an electron conductor in the metallic sense. (If the spectral shifts are great enough in the bimolecular complexes $D \cdot Car$ or $Car \cdot A$, these could also be energy sinks, with a similar but delayed electron mediation—the carotene storing the excess or defect of charge from the first transfer until it comes in contact with the second reactant, as chlorophyll is sometimes sup-

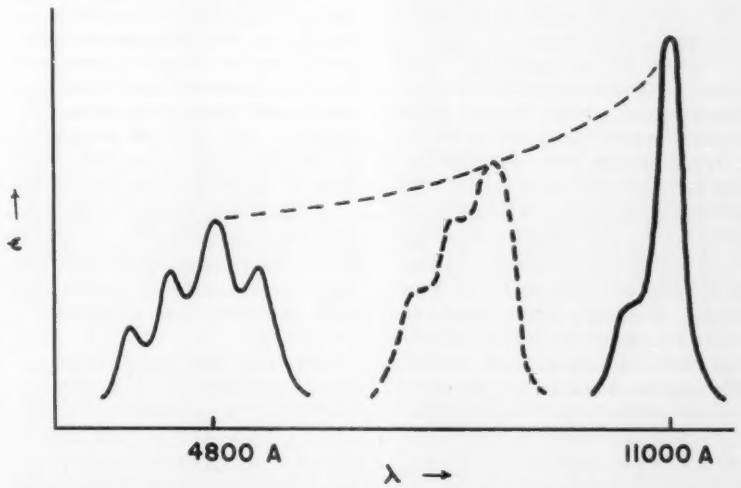

Fig. 1. Predicted change in the spectrum of a 22-atom conjugated chain from the case of one resonance structure dominant (left, carotene) to the hypothetical extreme case with two equivalent "isoenergetic" resonance structures in the ground state (right, very strong $D \cdot Car \quad A$ complex).

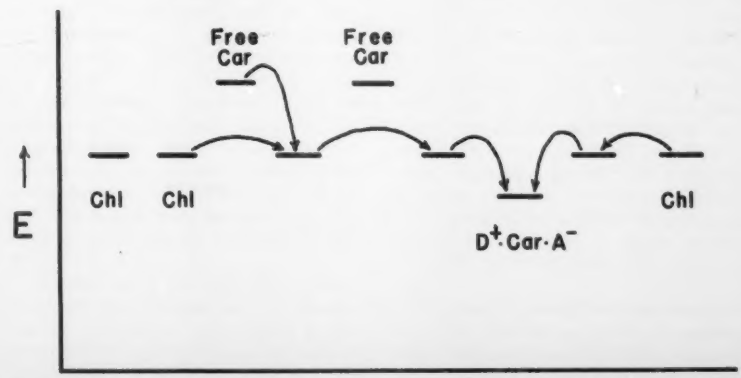

Fig. 2. The $D \cdot Car \cdot A$ complex as the possible energy sink for the excited states (singlets or triplets) of a large chlorophyll-carotenoid system containing donors and acceptors.

posed to do. But the shifts would be smaller and the reactions less rapid and specific than in the trimolecular case.)

There are two auxiliary possibilities that also deserve mention. First, if its energy is inadequate for separation of D^+ and A^-, the $D^+ \cdot Car \cdot A^-$ excited complex might store the energy for a short time in the triplet state, awaiting the arrival of a second quantum, just as chlorophyll itself is assumed to do in the Franck two-quantum hypothesis (*19*). The $D^+ \cdot Car \cdot A^-$ triplet might also be able to satisfy the requirements for the "first reservoir" of Bassham and Shibata (*20*). Second, the question is still under debate as to what the initial donors and acceptors are in photosynthesis; but if chlorophyll itself were the donor or acceptor in a carotene mechanism (*11*), the shift in the chlorophyll spectrum (*2, 9, 10*) and the quenching of its triplet (*4*) in the presence of carotene would both be accounted for. But the acceptability of the main mechanism described here is independent of whether these auxiliary possibilities prove acceptable or not.

Special features of the pathway. Note that the postulated energy-transfer steps here are of types that can have a quantum efficiency near unity; but the energy is not transferred to inadequate donors or acceptors or to random $D^+ \cdot A^-$ complexes in the solution. It is not transferred to just any carotene molecules, but only to those that are ready for the reaction. The reactants are preset, and the energy does not have to be held for the completion of diffusion-controlled steps. (While one quantum is being utilized at one site, these steps can be proceeding in parallel, as D and A molecules are joining up at other carotene sites.) The D^+ and A^- products of the reaction are also separated in space, so that the probability that they will both return or recombine to give a back reaction is smaller than it would be for a bimolecular electron transfer. If the carotenoids are part of the primary sequence, these features of the proposed process would be especially interesting in the light of the well-known evidence that there is a "photosynthetic unit," in which all of some hundreds of chlorophyll molecules can transfer their absorbed energy to a single reaction site; for any such unit would seem to require just this kind of efficiency and specificity.

Possible Tests

It is not easy to say what experiments would be best to test this picture. If there is only one $D \cdot Car \cdot A$ complex per photosynthetic unit at a given time, its spectrum would probably be too weak to detect directly, especially since we cannot predict exactly where its absorption maximum would be. Flash studies and paramagnetic studies might be similarly hampered. And while the fluorescence and phosphorescence of the chlorophylls would be quenched by this process, this is necessarily true of any efficient photochemical process. Moreover, complexes are always labile, and their weak binding energy in the ground state makes it hard to do convincing tracer or substitution experiments.

Probably the best tests will be the examination of simple polyene solution systems. If $D \cdot Car \cdot A$ complexes with a shifted spectrum can be found there, a number of energy-transfer experiments suggest themselves. Such studies will at least confirm or refute the possibility of applying this picture to photosynthesis in living systems, and they may suggest more direct ways to check the mechanism. Obviously they may also be relevant to the spectral shifts and transfer processes of the polyene visual pigments (*21*).

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21. I am indebted to Dr. B. L. Strehler (now of the Gerontology Branch, NIH, Baltimore City Hospital) for calling my attention to the problem of explaining theoretically how a chlorophyll-carotene system might work.

News of Science

Congressional Atomic Energy Group Studies Waste Disposal Problem

In public hearings that ended early this month the Joint Committee on Atomic Energy of Congress examined the problem of disposal of industrial radioactive wastes. The hearings were part of a series on radiation hazards begun by the Joint Committee in 1957; previous inquiries have been held on reactor hazards and fallout.

In his opening statement Representative Chet Holifield (D-Calif.), chairman of the subcommittee on radiation, explained the origins of the recent hearings.

"The subcommittee and staff became particularly interested in the problem of waste disposal during the hearing in 1957 on fallout from weapons tests. Several of the experts who testified stated that the hazards of radioactive wastes from the peacetime atomic power industry will be far greater than fallout from weapons tests. While radioactive wastes from atomic reactors are obviously much more subject to control than fallout from weapons tests once released, we want to see what the nature and magnitude of the production of radioactive wastes will be, and how good the methods of waste disposal are and will be.

"Indeed, the 'waste disposal' problem may be just as neglected, from a public standpoint, as fallout was in 1954. I hope that we will not require a series of incidents such as the contamination of the Japanese fishing boat, the Lucky Dragon, from the Marshall Islands test, to focus public attention on the waste disposal problem."

The public sessions, which ran for 5 days, were held in the Capitol in the old Supreme Court Chamber, an ornate room with a sculptured ceiling that curved down from the high wall in front of which the committee's chairs were placed. Witnesses from government, industry, and the universities offered testimony on the six major aspects of the waste disposal problem that the subcommittee had outlined. The first three aspects discussed (i) sources, quantities, and characteristics; (ii) current operational procedures and methods utilized in the collection, handling, and processing and disposal of waste material; and (iii) past and current work on the disposition of wastes. During the fourth and fifth days papers were presented on three other areas of the problem. These were (iv) problems of future quantities of waste and the effect of these, from the point of view of economics, on the future development of atomic energy; (v) activities of federal and state agencies in regulating disposal of radioactive wastes; and (vi) international aspects.

The report of the committee, which, according to J. A. Lieberman of the Atomic Energy Commission, will be the most complete compilation ever made of information on the waste-disposal problem, will be published by the Government Printing Office sometime in the latter half of March. Papers from individuals and organizations will be accepted by the committee until 15 February.

Public Safety

During the first two days of the hearings a number of points were made that received support from all of the persons offering testimony. The first of these was concerned with the interagency cooperation that has characterized the work done on the problem. Witnesses from the various atomic energy facilities referred again and again to the close working relationships that have been developed between plant personnel and the various municipal, state, and governmental agencies with which they have had contact. Safety of the public—for example, in communities downstream from a plant such as Hanford, Wash., on the Columbia River—has been the primary concern of the agencies dealing with waste disposal. Jurisdictional matters, as one witness said, were discussed after the work of protecting the public had been done.

A second point was the general praise given by the witnesses to the Atomic Energy Commission for its handling of a very difficult and new problem. Almost every witness who had not mentioned this in his presentation was interrogated on this point by the committee. The testimony was to the general effect that the commission had done an excellent, responsible job, both in the matter of protecting the public from the hazards associated with the waste problem and in disseminating information on waste disposal to all agencies working with the problem.

A third general point, again one specifically drawn out by the questions of committee members, concerned public safety. There was agreement that there had been no hazard to the public from industrial radioactive wastes since the inception of the atomic energy program in this country. One committee member, trying to conceive of a possible weak point in the current disposal system, brought up a case in which some external force might destroy one of the storage tanks containing radioactive waste. The witness agreed that this would cause a great hazard. The point that became clear during the testimony was that, given the fact that to date there is no perfect solution, the measures that have been taken are the best and most reliable that can be devised.

A last point, an overriding consideration, is that despite the safety record and the techniques that are now in use, the problem of waste disposal has not been solved. Rather, it has been handled as well as it could have been, given the present state of knowledge and experience. In conjunction with this, the further point was made that the problem will take on increasing seriousness as the use of atomic energy increases. Representative Holifield, quoting an article by Wallace de Laguna, dramatized the problem in this way: ". . . the quantity of the single isotope of strontium-90 produced will be so large by the year 2000 that roughly 16 million cubic miles of water would be needed to safely dilute it. That's more fresh water than there is in the world, including the polar icecaps."

Testimony

The amount of testimony given before the subcommittee on radiation precludes full presentation here. However, the nature and general outline of the hearings can be indicated by the four points given above and by selection of certain passages from the papers presented by witnesses. Of particular value here is the paper of the first witness, Abel Wolman, of Johns Hopkins University. Like the other speakers, he had been requested to present his testimony in terms understandable to the laymen and Congressmen.

"Up to the present time the management of radioactive waste materials, under the continuing and careful scrutiny of the Atomic Energy Commission, has followed two general precepts for the protection of man and his environment

in their ecological associations. The safe application of both of these precepts still remains the central theme of the Atomic Energy Commission policy. These precepts are: with high-level radioactive wastes, concentrate and contain; with low- and intermediate-level radioactive wastes, dilute and disperse to nature."

"Additional significant differences from more orthodox industrial wastes are the long half-lives and the damaging properties to human tissues of certain of the materials produced. The supervision and control of some of these wastes, therefore, must be viewed, not from the standpoint of temporary expediency, but from the necessity of guaranteed supervision and control, in some instances for hundreds of years. It is obvious that such a contingency, arising from this novel industry, places a major responsibility upon and challenge to all the public and private agencies dealing with the problem.

"The protection of the public health and of the total natural resources of this and of every country entails a greater depth of continuing responsibility than for any other industrial waste hitherto confronting society."

"Various estimates have been made of high-level wastes which will result from future nuclear power generation, based on estimates of the extent of such future nuclear power production, fuel irradiation levels, and unit volumes of waste per unit of fuel processed. Without attempting the role of a prophet, and on the assumption that nuclear power generating capacity may attain in 1980 something of the order of 1.1×10^5 megawatts of heat, the accumulated waste volume in gallons will be of the order of 10^8. The total fission product activity in curies will then be approximately 10^{11}."

"All of these figures simply demonstrate that the industry will be confronted increasingly with a waste-disposal and management problem, which one is unlikely to escape in the present state of the art. Although a great deal of attention is now being paid to this problem, to many industrialists the problem of waste disposal appears to be nonexistent. The reason for this happy state of mind lies in the fact that under present procedures the Atomic Energy Commission holds itself responsible for the handling of these most difficult materials."

Prospects for the Future

"The technical feasibility of direct disposal of high-level wastes into specific geologic formations is sufficiently clear to drive one toward at least limited application of such a procedure, either in salt domes or salt beds, in deep, isolated basins, in shale formations or even in porous formations. It would certainly be inadvisable to move to these presumed solutions to the problem without far more extensive exploration and pilot-plant application than are yet at hand. The engineering and economic issues involved are still in the very preliminary stages."

"The disposal of high-level wastes in the ocean has had much discussion. It is not unfair to conclude in the present state of understanding of the ocean that such a procedure is unlikely to be sanctioned for some years to come. So many unknowns and unpredictables with respect to oceanic behavior still remain that management and government will be driven to the safer practice of keeping high-level radioactive materials under more obvious scrutiny and control than would be provided by most ocean disposal procedures.

"Sufficient has already been said in this presentation to make clear that the rapid development of the atomic energy industry is in no small measure contingent upon more prompt and more complete answers to the waste-disposal problem. If the situation is somewhat pessimistic, it is only because sufficient energies have not yet been expended in developing economic and safe improvements in waste handling and disposal processes. Progress in this field is unlikely to occur if the problems are not realistically confronted. It is a tribute to the Atomic Energy Commission operations that so many *ad hoc* solutions have been provided which have safeguarded the public during the last 10 to 15 years."

"Much thought will be required in the foreseeable immediate future as to the best way of continuing long-term responsibility for disposal of radioactive materials. It is not inevitable that in this responsibility government will always have to play a dominant role, except in a supervisory capacity. Industry must increasingly assume responsibility for the physical operation of waste-disposal plants under the criteria to be established promptly by government. The selection of sites for nuclear energy facilities is closely related to waste-handling and -disposal operations. The recognition of this close relationship is not yet dramatically obvious to many individuals concerned with the development of this industry."

Science in the U.S.S.R.

The Soviet Union continues to play a leading role in science, although occasionally the government's actions in the field are puzzling. One piece of impressive news is the confirmation of the recent report that there is activity on the moon.

Lunar Eruption

Last fall Nikolai A. Kozyrev of the Crimean Astrophysical Observatory at Partinzanskoye announced that he had seen what he believed to be a lunar volcanic eruption. But Western scientists were skeptical. The moon has long been considered a dead body, both biologically and volcanically, and in the 350 years that astronomers have been making telescopic studies, there has been no real evidence to the contrary.

Now Kozyrev has published an account of his observations, accompanied by supporting spectrograms, in the February issue of *Sky and Telescope*, a journal edited at Harvard College Observatory in Cambridge, Mass. According to the editors of the journal, which is not an official organ of the observatory, Kozyrev's findings "seem incontrovertible." The paper was translated for publication by Luigi G. Jacchia, an observatory staff member.

Kozyrev saw the lunar activity while watching a crater known as Alphonsus. His interest in Alphonsus had been stirred by some photographs taken by an American astronomer, Dinsmore Alter, which had suggested the presence of a gas cloud covering the floor of the crater.

On 3 November at about 8 P.M., Kozyrev noticed that the 4300-foot peak in the center of Alphonsus had become blurred and taken on "an unusual reddish hue.' He thought this was caused by a change in the earth's atmosphere, and after making a spectrogram, he swung his 50-inch reflector telescope away. At 10 P.M. he returned to Alphonsus and made a 30-minute exposure. This time the central peak was extraordinarily bright and white, but as he looked through the eyepiece he noticed a marked drop in the brightness, and the peak resumed its normal appearance. He then halted the exposure and made a third one. The first spectrogram, when developed, proved to be normal; the second showed the bright light characteristic of an incandescent cloud containing carbon, such as is typical in volcanic eruptions on earth. This light ended abruptly in the spectrogram and was absent in the last exposure. In his article Kozyrev says:

"It is possible that the observations [of volcanic activity] just described will be unique for some time to come. But the existence today of internal energy and the possibility of orogenic [mountain-building] processes on the moon seem to have been established.

"The coincidence of the observed phenomenon with the position of the central peak can hardly have been accidental, and may indicate that the basic relief of the moon originated from within rather

than from the impact of giant meteorites. The low heat conductivity of the lunar surface layers may result from the porous character of volcanic material rather than from a dust layer."

Statements by two leading astronomers appeared in the *New York Times*. Donald H. Menzel, director of the Harvard College Observatory, accepts the evidence of the lunar activity but is not persuaded that it is volcanic in origin. He commented that Kozyrev has provided the first definite indication of the existence of a lunar atmosphere composed of rarefied gases. He explained further that leakage of gases from the interior of the moon could be the method for replenishing such an atmosphere.

Lloyd Motz, associate professor of astronomy at Columbia University, also accepts the basic finding that there is activity on the moon, but he, too, doubts the volcanic nature of the eruption. He pointed out that the moon may have very much more uranium than the earth, if it consists of rock structure all the way down. In that case, the radioactivity of the uranium below the surface would produce a temperature gradient, which, in turn, might lead to the ejection of hot materials in volcano-like activity. However, Motz feels that while some surface features of the moon may be the result of internal energy, most of them are the result of the impact of meteorites.

10-Bev Accelerator

Another significant development in Soviet scientific news is the announcement of the successful operation of a synchrophasatron that is the most powerful in the world. The new facility, located at the Joint Institute for Nuclear Research at Dubna, 80 miles north of Moscow, can accelerate protons from the hydrogen atom to an energy of 10 billion electron volts. This is a much higher energy than that generated by the similar installation at Berkeley, Calif., which has achieved a peak of 6.3 billion electron volts.

During a press tour of the Dubna center on 23 January, visitors also saw a new, two-story-high synchrocyclotron that accelerates protons and neutrons to an energy of 680 million electron volts. In addition, they had an opportunity to talk with Bruno Pontecorvo, the Italian-born nuclear physicist who left Britain's research center at Harwell in 1952 to go to the U.S.S.R. He told reporters that Soviet science, because of the social system in the U.S.S.R., was more advanced than that of the West.

Lysenko Reinstated

If Kozyrev's work and the new accelerators are demonstrations of the effect of the Soviet social system, so, too, is the third recent event in Soviet science—the reinstatement of geneticist Trofim D. Lysenko to a pre-eminent position in his country's biology. Lysenko, who was a powerful figure in Soviet science under Stalin, holds that in some cases acquired characteristics can be passed on to future generations. His views are in sharp conflict with those of Western geneticists. In recent years Lysenko's influence appeared to have waned, and Soviet scientists who opposed him were able to voice their criticisms with impunity. Then last summer the tide seemed to turn, as evidenced by last-minute changes in the character of the U.S.S.R.'s participation in the International Congress of Genetics in Montreal. Just before the meeting was to open, cancellations were received from several scheduled speakers, and the Soviet delegation submitted further titles of papers to be delivered, so that the Soviet contributions to the meeting were predominantly from the controversial Lysenko school. As a result of the program changes, the Permanent International Committee on Genetics Congresses presented a resolution at the final plenary session of the Montreal congress that expressed concern and sympathy for "all scientists who may have been prevented from attending the Congress by their governments."

Recent events indicate that the International Committee's point was well taken. The opposition to Lysenko has centered in the Soviet *Botanical Journal*. Although it has been evident for some time that Lysenko had found a new supporter in Khrushchev, the journal has continued to publish papers that attack Lysenko's work. However, just before the Soviet Communist party's recent Central Committee meeting, the official government organ *Pravda* severely criticized the *Botanical Journal's* repeated exposures of Lysenko.

When the Central Committee met, Lysenko was allowed to address a session and criticize his detractors. In response, Khrushchev said that the editors of the *Botanical Journal* must be changed. A verbatim account of the committee's proceedings, as it appeared in the British publication, the *Manchester Guardian*, follows:

"Mustafaev [Azerbaijan party secretary]: The situation in biology is especially bad, as was shown in the *Pravda* article which referred to the incomprehensible behaviour of the *Botanical Journal* and of some of our scientists. Instead of criticising each other in a businesslike and scientific way and pointing out faults, the affair has taken on an insulting tone.

"Khrushchev: We must take a good look at the staff. Evidently people were selected for the editorship who are opposed to Michurinist science. While they remain nothing will change. They must be changed and others put in—real Michurinists. Here lies the basic solution to the question.

"Mustafaev: Nikita Sergeyevich, it is not only this journal that uses this tone. Sometimes scientists who are also party members give no thought to how they should conduct themselves. Not long ago unpleasant rumours reached me that our delegation in China, among whom there were some biologists, declared that Comrade Lysenko was finished now, not only in theory but also in fact.

"Khrushchev: It was Tsistin who said that. [Tsistin, a noted Soviet biologist, has been vice-chairman of the Academy of Agricultural Sciences and director of the Moscow Agricultural Exhibition.]

"Mustafaev: That's bad. If they have had personal relations, it still does not give anybody the right to deprecate the achievements of our science.

"Khrushchev: He should have been asked at a party meeting why he had spoken in this way, and he should have been made to answer in his capacity as a party member. [Cries of 'Hear! Hear!']"

On 20 January, Moscow radio reported that the *Botanical Journal's* editorial board had been dismissed and that the U.S.S.R. Academy of Science's Biological Department was to be "improved." V. N. Sukachev, an academician, was replaced as editor of the *Botanical Journal* by V. F. Kuprevich, president of the Byelo-Russian Academy of Sciences. Further, at a joint meeting of the Soviet Academy's Presidium and Biology Department, V. A. Engelhardt, secretary of the Biology Department, formally acknowledged that criticism of his department had been "correct."

Pathologic Effects of Radiation

The Committee on the Pathologic Effects of Atomic Radiation of the National Academy of Sciences–National Research Council has issued a commentary on the recent report of the United Nations Scientific Committee on the Effects of Atomic Radiation [*Science* **128**, 402 (22 Aug. 1958)]. An excerpt of the commentary follows.

"In those portions of the United Nations report that deal with pathologic effects there are several points on which we are not entirely in agreement and are constrained to make clear our position. These minor points should not cloud our general agreement with and admiration for the report of the United Nations Scientific Committee. We believe, however, that it would be useful to make the following particular comments:

"1) The question of induction of leukemia or other types of cancer in man by very small doses of radiation has been treated in the United Nations report to

suggest that the hypotheses of linearity and threshold effects as applied to the behavior of somatic cells have equal likelihood of validity. Our committee inclines to the view that many forms of cancer, including leukemia, arise through a more or less complex series of responses. While somatic mutations may be included among these, it seems doubtful that a strict linearity analogous to that seen in the genetic effects of radiation is as likely to hold in the case of these conditions. We note that there is a considerable body of experimental evidence favoring nonlinearity in specific instances. Also, the report seems largely to emphasize the two extreme possibilities, that of a linear relation and of a threshold, and gives little attention to nonlinear relations. It is recognized that large-scale definitive experiments and demographic observations are needed since they may be of help in resolving these questions. . . ."

At a press conference held at the National Academy when the commentary was released, Shields Warren, chairman of the Committee on Pathologic Effects, announced that the committee was planning to propose a 20-year demographic study of the effects of radiation; the proposal would be presented to Congress within a year. Under the plan, which Warren estimated would cost $750,000 to $1 million a year, two groups of about a million persons each would be studied. One group would be made up of those living in an area known to have a high radiation incidence from cosmic rays, such as the Colorado plateau; the other group would consist of those living in a sea-level area of low radiation incidence, probably on the West Coast.

Science Talent Search

Forty high-school seniors, picked from a field of over 28,000, have been named winners in the 18th nationwide annual Science Talent Search. Nine girls and 31 boys have been awarded all-expense trips to Washington, where they will compete for $34,250 in Westinghouse scholarships and awards during a 5-day Science Talent Institute beginning 26 February.

Begun in 1942, the Science Talent Search is conducted by Science Clubs of America through Science Service, Washington, D.C. The Westinghouse Educational Foundation, supported by the Westinghouse Electric Corporation, sponsors the program.

This year's winners come from 17 states. New York continues to lead all other states in the number of winners produced, six boys and three girls; six of the winners come from New York City and vicinity. Illinois placed second with four.

Proconsul in Uganda

Proconsul, an Old World primate from the Lower Miocene of Kenya, was first described by Hopwood in 1933. This animal is particularly interesting in that, although its teeth exhibit specializations in the direction of the modern African anthropoid apes, the remainder of its known structure, including its brain (as determined from an endocranial cast), skull, and limbs, is much more generalized. Indeed, from animals with extremities of this sort there could have evolved, on the one hand, the modern anthropoid apes with their highly specialized limbs adapted to bimanual, arboreal progression and, on the other hand, the immediate precursors of the bipedal, terrestrial Hominidae [Clark and Leakey, *The Miocene Hominoidea of East Africa* (*Fossil Mammals of Africa,* No. 1) (British Museum, Natural History, London, 1951); Straus, *Am. Anthropologist* **54**, 257 (1952); Straus, in *Anthropology Today* (University of Chicago Press, Chicago, Ill., 1953), p. 77]. Thus, although its dental specializations apparently disbar it as the common ancestor of the Hominoidea (= anthropoid apes and man), *Proconsul* does provide a glimpse of what may well have been a critical, basic stage in hominoid evolution.

It therefore is of great interest that W. W. Bishop [*Nature* **182**, 1480 (29 Nov. 1958)] has recently reported the presence of an undoubted lower right second molar tooth of *Proconsul nyanzae* (the intermediate-sized of the three known species of *Proconsul*) among a rich and varied mammalian fauna from the Lower Miocene of Napak, Uganda. *Proconsul* hitherto was known from 10 localities in western Kenya, but this is the first record of its occurrence in Uganda. Since this creature evidently had a rather wide distribution, it may be hoped that future exploration will unearth some of the parts of its skeleton that are now missing and which are needed to establish its precise taxonomic status.—W. L. S., Jr.

Statistical Research Monographs

The Institute of Mathematical Statistics and the University of Chicago have established a series of publications entitled *Statistical Research Monographs.* The primary purpose of this series is to provide a medium of publication for material of interest to statisticians that is not ordinarily provided for by existing media. It will help fill the gap between journal articles and textbooks or treatises. Some of the kinds of publications envisaged are as follows.

1) New research results too lengthy for the usual journal article. In particular, authors will have ample scope for detailed exposition of their findings.

2) Research results of interest in both theoretical and applied statistics. At present authors of such material frequently find it necessary to publish part of their results in a theoretical journal and part in an applied journal.

3) Expository monographs in particular areas of statistics.

4) Discussions of statistical problems and techniques in particular areas of application.

The editorial board consists of David Blackwell (University of California), William G. Cochran (Harvard University), Henry E. Daniels (University of Birmingham), Leo A. Goodman (University of Chicago), Wassily Hoeffding (University of North Carolina), Jack C. Kiefer (Cornell University), and William H. Kruskal (University of Chicago). Authors are invited to send manuscripts and correspondence concerning the series to Leo A. Goodman, Department of Statistics, University of Chicago, Chicago 37, Ill.

Summer Conferences for College Teachers

The National Science Foundation has announced the award of grants totaling approximately $247,000 to 19 colleges and universities for an experimental program of Summer Conferences for College Teachers. These conferences are directed toward strengthening teachers' mastery of the newer developments in science and mathematics and toward increasing their capacity as teachers. The shorter length of these conferences, 1 to 3 weeks, as compared with the more familiar summer institutes of 4 to 12 weeks duration, will enable college teachers to familiarize themselves with recent advances in their specific fields. Association with colleagues from other areas of the country will be valuable to the participating college faculty members.

Under the new program, some 550 college teachers will receive financial support in the form of stipends up to $15 per day plus an allowance for travel. Stipend holders will not have to pay any registration fees or tuition. The conferences cover nine major subject-matter areas.

Participants will be chosen by the conferences, not by the National Science Foundation. Inquiries and applications for participation should be addressed to directors of the individual conferences named in the following list. Early inquiry is advised.

Biophysics. Yale University, New Haven, Conn. (Ernest C. Pollard, Biophysics Department).

Basic concepts in physical science. Georgetown University, Washington,

D.C. (Ralph S. Henderson, Department of Physics).

Radioisotopes. Cornell College, Mount Vernon, Iowa (Cecil F. Dam, Department of Physics).

Radioisotopes and tracer methodology. University of Maryland, College Park (Sitarama Lakshmanan, Department of Chemistry).

Structural chemistry. Tufts University, Medford, Mass. (M. Kent Wilson, Department of Chemistry).

Engineering graphics. University of Detroit, Detroit, Mich. (Paul M. Reinhard, Department of Engineering Graphics).

Analog computation. Michigan College of Mining and Technology, Houghton (Kenneth M. McMillan, Department of Mathematics).

Psychology. University of Michigan, Ann Arbor (Wilbert J. McKeachie, Department of Psychology).

Geobotany. University of Western Michigan, Kalamazoo (Harriette V. Bartoo, Department of Biology).

Analytical chemistry. Carleton College, Northfield, Minn. (Richard W. Ramette, Department of Chemistry).

Genetics. Long Island Biological Association, Cold Spring Harbor, N.Y. (Arthur Chovnick, Biological Laboratory).

Chemical instrumentation. New York University, New York (S. Z. Lewin, Department of Chemistry).

Botany. University of North Carolina, Chapel Hill (Victor A. Greulach, Department of Botany).

Process control theory. Case Institute of Technology, Cleveland, Ohio (James R. Hooper, Jr., Director of Special Programs).

Digital computers. University of Oklahoma, Norman (William Viavant, Director of Scientific Computations).

Geology. Oregon State College, Corvallis (W. D. Wilkinson, Department of Geology).

Ecology of fresh-water organisms. University of Pittsburgh, Pittsburgh, Pa. (C. A. Tryon, Jr., Department of Biological Sciences).

Plant biochemistry. Institute of Paper Chemistry, Appleton, Wis. (Elwood O. Dillingham, Department of Chemistry).

Inorganic chemistry. University of Wisconsin, Madison (Edwin M. Larsen, Department of Chemistry).

Liability for Nuclear Accidents

Paul Ruegger of Switzerland, a member of the Permanent Court of Arbitration at The Hague, has been appointed chairman of an International Atomic Energy Agency panel to consider problems of third-party liability in the field of atomic energy. The panel will be composed of representatives of nine countries which are members of the agency.

The lack of adequate rules and accepted definitions of liability in the case of nuclear accidents constitutes a serious retarding factor in the growth of the atomic energy industry. This is true in national contexts but is still more serious in bilateral or truly international operations such as those carried out under the auspices of the International Atomic Energy Agency. The problem will grow even more complicated if national legislatures adopt different solutions. The initial program of IAEA therefore stresses the need for efforts to establish international standards and definitions of areas of responsibility which would do much to harmonize national practices which are now being formulated in many countries.

The panel will be called together to initiate studies and international action in the field and to propose solutions to the many problems as speedily as possible. It will be left to the panel to consider whether international recommendations or specific steps toward an international convention promise the best results.

News Briefs

Joint hearings on defense preparedness and space exploration were held recently by the Senate Preparedness Subcommittee and the newly formed Committee on Aeronautics and Space Sciences. Lyndon B. Johnson, Senate majority leader, is chairman of both groups. The inquiry was based on two basic themes: (i) Is the United States doing everything that it reasonably can and should to insure the defense of this country and its allies against military aggression? (ii) Is the United States doing everything it reasonably can and should in the exploration of outer space?

The preparedness subcommittee held the widely publicized hearings on missiles last year, following the Soviet Union's launching of the first sputnik on 4 October 1957.

* * *

Measles deaths during 1957 outnumbered deaths caused by poliomyelitis—the first time since 1944 that this has occurred. According to Public Health Service figures, in 1957 there was an estimated total of 410 measles deaths, compared with 220 from poliomyelitis. The 1956 totals were 530 from measles and 566 from poliomyelitis. In 1944 there were 1923 deaths from measles and 1361 from poliomyelitis.

* * *

The Professional Group on Information Theory of the Institute of Radio Engineers, 1 E. 79 St., New York 21, N.Y., has announced a new affiliate plan. Under the plan, members of selected technical societies are entitled to become affiliated with and receive the publications of some of the professional groups of the IRE without having to join the IRE itself. They need only pay the regular professional group dues, plus $4.50, rather than the much larger fee ($10) for full institute membership. The regular PGIT dues are $3.

* * *

The General Electric Lamp Division, Cleveland, Ohio, has dedicated its new Lamp Research Center at Nela Park and thus launched an intensive program aimed at "advancing the frontiers of knowledge of light production and its effect on all living things." Carl L. Olson, manager facility, said it was built to house an organization of some 250 research people who will not be bound by current thinking as regards the source, form, fabrication, operation, and application of light.

* * *

The U.S. Department of Interior has announced the establishment of a Branch of Archeology, which has started operations with a staff of seven employees. John M. Corbett is head of the branch, which is a part of the National Park Service's Division of Interpretation. Corbett has been with the service since 1957. The new unit's responsibilities include both the locating of historical sites of possible national significance and the uncovering of evidence of the country's pioneer settlers.

Grants, Fellowships, and Awards

Allergy. The Scientific and Educational Council of the Allergy Foundation of America has announced the availability of a limited number of quarterly or summer scholarships at $500 each in approved medical schools in the United States and Canada. These scholarships, which are to be for a minimum of 8 weeks of training in clinical and research allergy, are available to students who have completed their second or third year in medical school.

Each medical school has been invited to submit the name of one applicant through the dean's office, with a letter from the dean in support of the candidate's application. Direct application from students will not be considered. All applications must be sent *before 1 March* to Dr. Robert A. Cooke, Chairman, Scientific and Educational Council, Allergy Foundation of America, 801 2nd Ave., New York 14, N.Y.

Chemistry. Nominations are invited for the $500 Dexter Award in the history of chemistry administered by the Division of History of Chemistry of the American Chemical Society. The award will be made on the basis of services

which have advanced the history of chemistry in any of the following ways: by publication of an important book or article; by the furtherance of the teaching of the history of chemistry; by significant contributions to the bibliography of the history of chemistry; or by meritorious services over a long period of time which have resulted in the advancement of the history of chemistry.

All information, in duplicate, should be sent to the secretary of the Division of History of Chemistry *by 10 March.* Information should be as detailed as possible and should include outstanding as well as minor contributions of the nominee. Failure to furnish full information may unintentionally penalize your candidate. The division secretary is Sidney M. Edelstein, Dexter Chemical Corporation, 845 Edgewater Rd., Bronx 59, N.Y.

Radioisotopes. The Atomic Energy Commission has announced the establishment of a new program of assistance to colleges and universities for education and training in radioisotope principles and technology. The new program provides for direct financial assistance to colleges and universities in obtaining demonstration apparatus, student laboratory equipment, and training aids needed to offer adequate laboratory course work in radioisotope technology. Examples of apparatus and equipment available under the program are as follows: ionization chambers; scintillation counters; scalers and ratemeters; radioactivity standards; Geiger-Mueller counters; gas flow counters; pulse analyzers; radiation sources; training aids that illustrate industrial applications of isotopes; and equipment for the safe storage, proper handling, and disposal of radioactive materials.

Requirements for submission of proposals for equipment grants under this program, and the criteria used in evaluating proposals, may be obtained from the Director, Office of Isotopes Development, U.S. Atomic Energy Commission, Washington 25, D.C.

Scientists in the News

CARL DJERASSI, an internationally known organic chemist and professor at Wayne State University, has won the $1000 Leo Hendrik Baekeland Award of the American Chemical Society's North Jersey Section. Djerassi, on leave of absence from Wayne, is vice president for research of Syntex, S.A., Mexico City manufacturer of hormones.

Djerassi has made important contributions to the synthesis of cortisone, other hormones of the steroid class, and antihistaminic drugs. He was coinventor of Pyribenzamine, one of the earliest antihistamines. He has described the chemical structure of numerous products derived from plants. His laboratory methods based on studies of optical rotatory dispersion have created a new field in physical organic chemistry and have provided scientists with a powerful new analytical technique. As director of research for Syntex, Djerassi played an important role in the development of a class of orally effective steroid hormones now being used for the treatment of female disorders, such as infertility.

GEORGE WALD, professor of biology at Harvard University, has been named recipient of the 1959 Rumford Premium of the American Academy of Arts and Sciences, Boston. The formal presentation will take place at a meeting of the academy on 11 March 1959. Wald is being honored with the $5000 award for his studies on the biochemical basis of vision. His research has centered on the chemistry of the visual process, with particular reference to the chemical changes that take place in the rods and cones of the eye following excitation by light quanta.

An award is being established in honor of MERVIN J. KELLY. Bell Telephone Laboratories and the American Institute of Electrical Engineers have announced the Mervin J. Kelly Award, a $1000 prize that will be given annually for achievement in telecommunications.

Kelly, formerly president of Bell Laboratories, is now chairman of the board. He will retire on 1 March after 41 years of scientific and administrative service with the Bell Telephone System. The first Kelly award will be made by the American Institute of Electrical Engineers in 1960. The award is being sponsored by Bell Laboratories but will be administered solely by the institute.

ALLEN E. PUCKETT, specialist in aerodynamics, has been named a vice president and director of the systems development laboratories of Hughes Aircraft Company (Calif.). Puckett joined Hughes in 1949 after 4 years as chief of the wind tunnel section of the Jet Propulsion Laboratories of California Institute of Technology. He served as head of Hughes' aerodynamics department of the guided missiles laboratories, then moved to the systems development laboratories as director of advanced planning and later as director of operations and associate director.

WILFORD R. GARDNER, physicist at the U.S. Salinity Laboratory, Riverside, Calif., will study in England and The Netherlands for a period of 1 year, under a National Science Foundation Senior fellowship award. He will spend 4 months at the Agricultural University at Wageningen, The Netherlands, where he will be associated with W. R. van Wijk in the study of the transpiration of water by plants, and 8 months at Cambridge University School of Agriculture in England with E. C. Childs, head of the Unit on Soil Physics. In both institutions, Gardner plans to study and conduct research on the physical processes in soil-plant relationships.

HERBERT A. SMITH, professor of education at the University of Kansas, has been appointed to head the U.S. Office of Education's new Division of Science, Mathematics, and Foreign Language. Smith has served as director of the program for educating science teachers at Kansas since 1953.

ALVAR P. WILSKA, on leave as professor of physiology, University of Helsinki (Finland), has been appointed visiting professor of cell research in the department of anatomy, Louisiana State University School of Medicine, New Orleans, for a period of 2 years. The primary purpose of the visit will be to complete the construction of a refined model of a "slow-beam," high-contrast electron microscope originally designed by Wilska. The instrument will be used to investigate new problems of cell research at the macromolecular level of organization.

ENOCH R. NEEDLES, consultant in civil engineering, has been reelected president of the Engineers Joint Council for 1959. AUGUSTUS B. KINZEL, vice president for research at the Union Carbide Corporation, has been elected EJC's vice president.

POLYKARP KUSCH, professor of physics and Nobel laureate, and CARL F. KAYAN, professor of mechanical engineering, both members of the Columbia University faculty, have each received a Great Teacher Award from the university's Society of Older Graduates.

The board of medical editors of *Modern Medicine* has announced the winners of the publication's ten annual Distinguished Achievement Awards. The 1958 recipients were chosen from hundreds of candidates nominated by deans of medical schools and readers of the journal. The award winners follow.

HENRY K. BEECHER, Henry Isaiah Dorr professor of research in anesthesia at Harvard University and director of anesthesia at Massachusetts General Hospital, for "zeal in improving methods of anesthesia and development of means for critical evaluation of new analgesic and anesthetic agents."

PAUL C. BUCY, professor of surgery at Northwestern University and president of the World Federation of Neurosurgical Societies, for "research elucidating the functions of the motor cortex and

leadership in clinical neurologic surgery."

NICHOLSON J. EASTMAN, obstetrician-in-chief of Johns Hopkins Hospital, for "pioneer studies on fetal oxygenation and on the etiology of cerebral palsy."

V. EVERETT KINSEY, assistant director of research at the Kresge Eye Institute and professor of ophthalmic chemistry at Wayne University, Detroit, for "fundamental work on retrolental fibroplasia in the newborn and on the basic physiology underlying development of glaucoma."

ROBERT F. LOEB, Bard professor of medicine at Columbia University and director of medical service at Presbyterian Hospital, New York City, for "investigations of electrolyte physiology and the adrenocortical relationship to salt and water metabolism and an outstanding career as teacher and clinician."

C. N. H. LONG, Sterling professor and chairman of the department of physiology at Yale University, for "basic contributions interpreting the control of metabolism by endocrine glands, particularly the adrenal cortex and anterior pituitary."

WILLIS J. POTTS, surgeon-in-chief of Children's Memorial Hospital, Chicago, for "significant advancement of surgical correction of congenital anomalies and his continuing influence in development of pediatric surgery."

TRACY J. PUTNAM, Cedars of Lebanon Hospital, Beverly Hills, Calif., for "a lifetime of fruitful endeavor in teaching and practice and neurologic research into the nature of convulsive disorders."

CECIL J. WATSON, head of the University of Minnesota department of medicine, for "his additions to clinical knowledge of liver function and the clarification of porphyrin metabolism."

W. BARRY WOOD, JR., vice president of Johns Hopkins University and Hospital and professor of microbiology, for "research concerning the pathogenesis of fever and leadership in exploring new methods of medical education."

SEVERO OCHOA, chairman of the department of biochemistry at New York University, and JOHN A. D. COOPER, professor of biochemistry and assistant dean of the Northwestern University Medical School, recently were honored by the University of Brazil with the degree of Doutor Honoris Causa. The honorary degrees were given to the two men in recognition of their help in establishing a radioisotope laboratory in the university's Institute of Biophysics.

ARTHUR V. PETERSON, nuclear engineer, has resigned as vice president of AMF Atomics, a division of American Machine & Foundry Company, and of AMF Atomics (Canada) Ltd., to establish Arthur V. Peterson Associates. The new firm, which will have headquarters in Westport, Conn., will provide atomic energy consultation to industrial management. Peterson has been associated with the development of atomic energy since his assignment to the Manhattan Engineer District at the time of its formation in 1942. From 1947 to 1953, he was head of the Atomic Energy Commission's fissionable material branch.

JOSEPH A. STERN, formerly associate professor, technology, at the College of Fisheries, University of Washington, has assumed the position of chief of the biochemistry unit, Branch of Space Medicine, Systems Management Office, Boeing Airplane Company, Seattle, Wash. He will continue as a lecturer at the college. JOHN LISTON, formerly of the Torry Research Station, Aberdeen, Scotland, who joined the faculty in 1957, will assume full responsibility for the technological program at the college.

Also at the College of Fisheries, ALBERT K. SPARKS, formerly chief biologist and assistant director of the Texas A. and M. Research Foundation's Marine Laboratory at Grand Isle, La., has joined the faculty as associate professor in invertebrate fisheries, replacing JAMES E. LYNCH, who retired as professor emeritus.

HOBART H. WILLARD, analytical chemist and professor emeritus, University of Michigan, will teach two courses at Valparaiso University next semester. Since he retired from the University of Michigan in 1951, Willard has spent each summer and fall investigating the separation of various compounds and elements at Los Alamos Scientific Laboratory. He devotes the spring months to teaching, usually changing universities each year.

JOHN H. LAWRENCE, director of the Donner Laboratory, University of California, Berkeley, recently lectured at the University of Bordeaux, where he received an honorary doctor of philosophy degree.

Recent Deaths

JEROME ALEXANDER, New York, N.Y.; 82; retired in 1951 as consultant chemist and chemical engineer; was decorated by the French Government in 1931 and 1936 for his services to science; author of *Colloid Chemistry*; 18 Jan.

Sir CLAUDE D. GIBB, Newark, N.J.; 60; British physicist and chairman and managing director of C. A. Parsons & Co., Ltd., and of the Nuclear Power Plant Company, Ltd., Newcastle-upon-Tyne, England; 15 Jan.

PEARL GREENBERG, Linden, N.J.; 65; chief clinical psychologist for the Veterans Administration in Newark; studied under Alfred Adler at the University of Vienna, Austria, where she received her Ph.D. degree; did psychological and social research for the Sociological Institute of Rutgers University and was clinical psychologist at Marlboro State Hospital; 4 Jan.

GUSTAV HIRSCH, Columbus, Ohio; 82; electronics engineer, who developed a successful television transmitter in 1928; president of the Skyway Broadcasting Company, and director of 13 independent telephone companies; 7 Jan.

WALTER J. MOXOM, Woolford, Md.; 74; meteorologist who retired in 1950 as director of the Weather Bureau's Regional Office, New York; headed the St. Louis Weather Bureau Office, 1938–41; assistant chief of the River and Flood Division, Central Office, Washington, D.C., 1930–38; had been connected with the Weather Bureau for 45 years; 13 Jan.

META P. NEWMAN, Cambridge, England; editor of *Nursing World* from 1928 to 1941; former president of the American Association of Women in Public Health, and former assistant director of health education for the National Foundation; author of two books on nursing; 15 Jan.

F. LANSING STEBBINS, Elizabeth, N.J.; 54; technical editor at the Volt Technical Corporation and formerly a technical editor in Navy and Air Force projects; 18 Jan.

HOLLAND N. STEVENSON, Pelham, N.Y.; 72; otolaryngologist who retired in 1956 after 36 years of practice; former director of otolaryngology at New Rochelle Hospital; professor of bacteriology and surgical pathology at Creighton University, 1918–19; taught pathology at Johns Hopkins University, 1912–15; 12 Jan.

JULIUS WYLER, Queens, N.Y.; 67; professor of applied statistics, Graduate Faculty, New School for Social Research, since 1942; before coming to the United States in 1941, he was assistant director of the Swiss Federal Statistical Office; 13 Jan.

EDWIN G. ZABRISKIE, New York, N.Y.; 84; professor of clinical neurology at the College of Physicians and Surgeons, Columbia University, from 1925 until his retirement in 1948; acting director of the Neurological Institute of New York, 1946–47; studied neurology at universities in Paris and Berlin; founder and diplomate of the American Board of Psychiatry and Neurology; former president of the American Neurological Association and of the Association for Research in Nervous and Mental Diseases; 13 Jan.

Book Reviews

The American High School Today. A first report to interested citizens. James B. Conant. McGraw-Hill, New York, 1959. xiii + 140 pp. Cloth, $2.95; paper, $1.

An Essay on Quality in Public Education. Educational Policies Commission. National Education Association of the United States, Washington, D.C., 1959. 31 pp. $0.35.

When James B. Conant resigned his appointment as ambassador to Germany, he began immediately, under a grant from the Carnegie Corporation, to study the American comprehensive high school. He and four collaborators visited 103 high schools, plus four city school systems, in 26 states. Information was gathered on course offerings, the courses actually taken by students of different ability levels, and the effectiveness with which the schools achieved different educational objectives.

Specialized institutions such as the Bronx High School of Science may be desirable under certain circumstances, but those circumstances are rarely found, and it is the public, comprehensive, general-purpose high school that serves most students of high-school age, be they brilliant or dull, academically highly motivated or restlessly waiting for the day when they can escape school and get a job. Conant undertook to find out whether this kind of school can "at one and the same time provide a good general education for *all* the pupils as future citizens of a democracy, provide elective programs for the majority to develop useful skills, and educate adequately those with a talent for handling advanced academic subjects."

Most high schools he studied were not doing an adequate job on all three tasks, but some were. These facts led him to conclude that no fundamental change in the prevailing structure of the American high school is necessary, but that improvements are widely needed.

In the same week that Conant's report appeared, there also appeared *An Essay on Quality in Public Education,* a statement by the Educational Policies Commission of the National Education Association and the American Association of School Administrators. The two documents reinforce each other. Both are intended to be read by members of school boards and by citizens interested in appraising the quality of schools in their own communities. Both are middle-of-the-road documents far from the extremist positions that have marked much of the recent debate over American education. On a number of recommendations, the two are in agreement. Both agree that the high school must provide general education for all, vocational education for some, and intellectually rigorous education for the academically talented. Both agree that there should not be separate tracks or programs, such as a vocational curriculum and a college preparatory curriculum, but rather that there should be a variety of academic and vocational offerings within which good guidance can provide an appropriate and individually selected program for each student. Both agree that much more emphasis should be given to the teaching of foreign languages and that those students who are able to do so should take three or four years of one language—enough to gain useful facility in reading and speech—instead of quitting after two years or instead of taking inadequate amounts of two different languages. Both agree that small high schools are inefficient and can be maintained only at extravagant cost or by sacrificing quality; to be efficient a high school should have a graduating class of at least 100 (Conant) or a total four-year enrollment of 500 (EPC); to the maximum extent possible smaller high schools should be combined into larger schools.

The two reports are similar in intended audience, in their basic point of view, and in several recommendations, but they differ sharply in the way in which the recommendations are presented and in the points given greatest emphasis. Under the heading "Prerequisites to High Quality in Education," the EPC report concludes with a formula for determining whether or not a school is adequately supported: "In a school district of adequate size the minimum annual per-pupil current expenditure needed today to provide a good educational program is about 12 percent of the salary necessary to employ a qualified beginning teacher in that district. . . . The minimum starting salaries for qualified beginning teachers in any community should equal the average of the salaries offered to new college graduates in that community." The implication is clear that if the community supports its schools this well, the community will have schools of high quality.

Conant agrees that more money is needed, but addresses himself to the ways in which that money should be spent and to specific changes in school organization and policy, some of which require no money at all. The 21 recommendations he collects in one chapter are down-to-earth proposals that could be put into effect next semester, next year, or year after next. On any scale of specificity, Conant comes out far in the lead. Perhaps this is a difference between an author and a committee. Conant could write what he thought, while the Educational Policies Commission could write only what was agreed to by 19 members. However one explains the difference, the Conant report provides much the better basis for community action. On too many points, I fear, people can agree with the words of the EPC report while holding quite different views of what those words mean. One can agree or disagree with Conant, but both sides will know precisely what they are talking about. The recommendations Conant makes are specific, and each has been found to work effectively in one or more high schools.

In a foreword to Conant's report, John Gardner concludes, "If I had to recommend a single piece of reading to all Americans who want to improve their schools, I would ask them to read this report." I agree.

DAEL WOLFLE
American Association for the Advancement of Science

Bacteriological Code, International Code of Nomenclature of Bacteria and Viruses. Edited by the editorial board of the International Committee on Bacteriological Nomenclature. Iowa State College Press, Ames, 1958. xxii + 186 pp. $3.50.

It has long been apparent that the bacteria and the viruses occupy an intermediate position between the plant and animal kingdoms, but the early suggestion that they be considered as belonging to a separate kingdom, the Protista, was never generally accepted. The formal classification of at least the bacteria as plants has not been seriously disputed, perhaps in part because the point appears of little importance to the majority of bacteriologists. At the same time, the limitations of the Botanical Code as applied to these forms has been clearly evident. This inadequacy, or rather the special requirements of a bacteriological code of nomenclature, was

recognized by the First International Microbiological Congress, in 1930, by the appointment of the first of a series of International Committees on Bacteriological Nomenclature, which have evolved the structure laid down in the present volume.

These committees and their subcommittees have done a magnificent job and have had the courage and initiative to depart from the Botanical and Zoological codes where it has appeared useful to do so. The resulting Bacteriological Code—although differing in certain respects, such as the definition of subgeneric taxa—has retained very many features of the Botanical and Zoological codes, and the three codes form a closely knit group.

The Bacteriological Code is described in four chapters devoted to general considerations, principles, rules and recommendations, and provisions for modification and amendment. Of these, the third chapter is by far the largest and consists of the rules of nomenclature, together with illustrative examples of their application and notations of their resemblances or differences from the corresponding rules in the Botanical and Zoological codes. The fourth chapter, although a part of the code, is a kind of constitution and by-laws for the International Committee on Bacteriological Nomenclature and its subgroups—a judicial commission and various taxonomic subcommittees. There is also an appendix, which includes a summary of usage in the transliteration of Greek words, a section on alternative spellings, a summary of opinions issued by the committee, and a list of conserved and rejected names.

The stated aim of the efforts of the committee is to provide a fixity of legitimate names, putting the nomenclature of the past in order and also providing a nomenclature for the future. Present nomenclature of bacteria—that inherited from the past—is unquestionably in a less than perfect state, having been derived by application of both botanical and zoological systems and seasoned with personal inspirations. At the same time its reformation results in a certain amount of trauma—that caused by a seeming fluidity, such as the shifting of the glanders bacillus from *Actinobacillus* to *Pfeifferella*, to *Malleomyces*, and back to *Actinobacillus*, and that occasioned by the disappearance of an old, well-established name in favor of a quite unknown name because of the discovery of an obscure note published in an obscure journal many years ago. In my opinion, the inclusion of a "grandfather clause" making legitimate current, generally accepted, nomenclature might have some small advantage.

The provision of a nomenclature for the future raises questions which have little or no precedent. It is now clear, for example, that the etiology of diphtheria is dual in nature in that the toxigenic bacillus is lysogenic, and the genetics of the relationship between the bacillus and the bacterial virus is only now being worked out; one wonders what is to become of the entity *Corynebacterium diphtheriae*.

The expressed views of the committee imply that the viruses are to be regarded as bacteria. The semantic aspect is no doubt of small importance, but the problems of nomenclature are immensely complex because they are inevitably taxonomic in nature. A promising start in this direction has already been made, and it may be anticipated that the committee will eventually be able to put this part of the microbiologist's house in order also.

WILLIAM BURROWS
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Man's World of Sound. John R. Pierce and Edward E. David, Jr. Doubleday, Garden City, N.Y., 1958. 287 pp. Illus. $5.

Acoustics may be one of the older and less popular branches of physics, but it has had its share of exciting developments in the past decade or so. In fact, the science of sound is a good example of a new trend—that of bringing man back into the picture. For several centuries the experimental and theoretical studies of sound have paid more attention to its propagation than to its generation and have usually stopped short of the eardrum. This was necessary as long as the physics of sound transmission was not well understood. But with the advent of the vacuum tube, exact measurement became relatively easy, and we are now in a position to study the broader subject of communication from man to man, of which physical acoustics is but a part.

It is in the fields of psychoacoustics, physiology of speech and hearing, and information theory that the most exciting advances have recently been made, not the least surprising development being that these new, nonphysical findings also are quantitative and amenable to mathematical representation. At present it is easier to predict theoretically the decrease in intelligibility of a lecture caused by the passage of a jet plane overhead than it is to predict, theoretically, the properties of a transistor.

Many of these new developments are reported in popular terms for the first time in *Man's World of Sound*. Here the nonspecialist can see how information theory is beginning to unify branches of physics, physiology, and psychology into a scientific study of man's auditory communication to man. The first third of the book deals with the physics of sound; the second third, with the physiology and psychology of speech and hearing. The final third discusses the newer concepts, such as that of intelligibility and information rate and the recent ideas about how speech may be encoded (that is, written down) automatically and how this code can be artificially transformed again into spoken words. The explanations are lucid; there are graphs but no equations.

One might wish that this book could become as popular as Rachel Carson's *The Sea Around Us*. Certainly the subject of oral communication, in its broadest sense, is as important to us as is the ocean; indeed, in the long run it may be as important as the subject of nuclear fission. But I doubt that *Man's World of Sound* will achieve this sort of popularity, for several reasons. For one thing, the book is somewhat spotty; chapters of clear and interestingly written exposition contrast with pages rather loaded with definitions and bald facts. For another, the authors have avoided those pseudophilosophical disquisitions on the inner meaning of things which so impress the more influential literary critics. It is also true that sound and speech are a little too commonplace to arouse a sense of wonder without resort to histrionics, which the authors have eschewed.

But these are minor faults (if they are faults at all). They should not deter the nonspecialist reader from learning about new and interesting developments in this important field of science and technology.

PHILIP M. MORSE
Department of Physics, Massachusetts Institute of Technology

The Scope of Physical Anthropology and Its Place in Academic Studies. A symposium held at the Ciba Foundation, 6 Nov. 1957. D. F. Roberts and J. S. Weiner, Eds. Published for the Society for the Study of Human Biology by the Wenner-Gren Foundation for Anthropological Research, New York, 1958. 66 pp.

The expansion of the scope and the widened interests of physical anthropology are succinctly and shrewdly discussed in this little symposium volume. The Ciba Foundation gathered together ten of Britain's foremost students of human evolution (Le Gros Clark, Penrose, Stevenson, Young, Mourant, Barnicot, J. S. Weiner, Oakley, Tanner, and Zuckerman), who have outlined their conception of the research and teaching obligations of the science. The diversity of research interests examined range from

J. M. Tanner's work on human constitutional variation and morphology to the experimental, laboratory studies of Barnicot on human pigmentation. There is an emphasis on morphological and anatomical studies which is not surprising, since Weiner, Le Gros Clark, and Tanner are the principal contributors within the field of physical anthropology. One welcomes the extended discussion of research in human population genetics and its role in our science, as presented by Penrose, Stevenson, Mourant, and Barnicot. The interests of the latter in laboratory and experimental studies is especially important at this stage in the growth of physical anthropology.

Two significant topics treated in the hortatory parts of the symposium are physical anthropology as a liberal discipline (J. Z. Young) and the design of "dream" curricula (Tanner and Weiner). Cellular biology—something of a fad at the moment—finds a place even here in a discussion of human biology and medicine (Tanner).

I am disappointed that this publication fails to note important new research areas in physical anthropology. The implications of recent discoveries in human biochemical genetics (the haptoglobin alleles, hemoglobin variants, Gm serum groups, beta-globulin alleles, white-cell antigens) for the training of students and the organization of laboratories are neglected. There is little awareness in the formal papers (the discussions are not included in the volume) that our discipline encompasses the genetics and morphology of nonhuman primates.

The symposium is directed particularly to British problems, and some of the proposals are not necessarily relevant outside Great Britain. Despite these reservations, this book ably demonstrates that British physical anthropologists living in the middle of the 20th century also practice 20th-century science.

JOHN BUETTNER-JANUSCH
Department of Anthropology,
Yale University

Amid Masters of Twentieth Century Medicine. A panorama of persons and pictures. Leonard G. Rowntree. Thomas, Springfield, Ill., 1958. xviii + 684 pp. $11.50.

The subtitle, "A panorama of persons and pictures," expresses fairly well the nature and content of this highly personalized book. The author, a distinguished physician and now emeritus professor, reminisces on men and events from associations developed during an academic career of some 50 years spent largely at Johns Hopkins Medical School and as chief of medicine at the Mayo Clinic. The attempt—apparently inspired by remarks in one of Winston Churchill's books—is to present the medical developments of the past half century from the viewpoint of a participant. In fact, quoting from Churchill, the author has sought to emulate the method employed by Daniel Defoe in his *Memoirs of a Cavalier,* where European military and political events of the first half of the 17th century are chronicled in the personal narrative of the fictitious "Col. Andrew Newport."

However, this method, to be successful, requires a great literary skill, deep critical understanding, and insight, and above all, the approach must be consistent. Unfortunately this work fulfills none of these requirements. At times it is frankly autobiographical, at others it interjects a series of thumbnail sketches of medical scientists—all of which tends to destroy coherency and relegates it to the category of reminiscences. Further, the work would have been greatly improved by good editing to rid it of repetitions, stylistic imperfections, and the large number of minor errors.

This is not to say that the book will not have appeal. A rich experience and wide knowledge of the medical field provides much of merit which is well presented and will interest both lay readers and members of the author's profession.

J. B. DEC. M. SAUNDERS
University of California
Medical Center, San Francisco

The Grafter's Handbook. R. J. Garner. Faber and Faber, London, ed. 2, 1958. 260 pp. 25s.

The second edition of this handbook retains its commendable objective in presenting the amateur or professional horticulturist with a simple, straightforward discussion of the science of grafting. That it has been brought up to date is evidenced by the inclusion of such topics as gibberellic acid, polyethylene, and mist propagation. The discussion of the recent use of such methods in horticulture and of their immediate application to grafting reflects the progressive attitude of the author. The same may be said of Garner's discussion of the various grafting methods used by virologists in indexing plants for the transmission of virus diseases.

The handbook begins with the occurrence of grafting in nature and antiquity. Succeeding chapters take up compatibility and cambial contact, rootstocks and their propagation, the collection and treatment of scion-wood, tools and accessories, methods of grafting, tree-raising in nurseries, and grafting-established trees, and there is a concluding chapter.

The reader is readily able to follow all instructions by means of 149 line drawings and photographs accompanying the text. A listing of 143 references is invaluable to the professional desiring more detailed information. Many English terms foreign to our horticultural language are defined in the glossary.

The recommended combinations of rootstock and scions cover only pears and plums in the three appendices. This section could be expanded to include other fruits and some of the commonly grafted ornamental woody plants. Information of this type would aid nurserymen in producing compatibly grafted plants of desired habit or vigor.

Garner is to be commended for his clear exposition of the subject-matter of this volume, which documents his 30 years' experience at the East Malling Research Station.

WILLIAM F. KOSAR
U.S. National Arboretum,
Washington, D.C.

The Physical Foundation of Biology. An analytical study. Walter M. Elsasser. Pergamon Press, New York and London, 1958. x + 219 pp. Illus. $4.75.

The scientific ideals and, to an important extent, the working structure of biology are governed by established tenets concerning its relations to physics. In this system of faith, physics is the paradigm of what is scientific; and biology at its contemporary best is a rather messy but hopeful kind of physics, the resultant of an inconvenient number of variables and the preoccupation of the better minds with the cleaner aspects of nature. Biologists may feel a bit uneasy about this, especially when they are reflecting upon important biological insights which owe little to physics and which may, as in the case of the evolutionary concepts, have had a considerable impact upon the physical sciences. But, as the cracker-barrel philosophy of biology has developed in the hands of biologists, it has seemed that the alternative would be vitalism, which has been demonstrably sterile.

So pretentious are the titles of books, that many readers encountering Walter Elsasser's *Physical Foundations of Biology* would probably expect to find still another tract on physical pie in the biological sky. This book, by a professor of theoretical physics at the University of California at La Jolla, contains more than the title promises, for it is not merely a highly critical examination of the question "can physics explain biology?" but also a reexamination of the foundations of physics in the light of those potentialities of the physical world that are realized in biological systems.

For, in the frame of Elsasser's analysis, vitalism could be worthy of consideration only if there were some likelihood that biological systems were not subject to the restrictions imposed by basic physical principles, and experiment has rendered this likelihood very small. The real question is not whether biological systems are material systems—they are—but whether they are *mechanisms*. He departs immediately from the seldom-criticized alternatives of vitalism and mechanism.

In fact, the question whether living organisms are mechanisms depends first on the construction of a rigorous and relevant definition of a mechanism; and it is perhaps the relevance, the rarest commodity in discourse about biology, that impresses a biologist most strongly about Elsasser's enterprise. The aspect of mechanisms chosen as most relevant is their performance as converters of information, and his approach is through Information Theory and related areas of physical science. Over a hundred pages devoted to a treatment of problems of feedback and control, information, and storage and memory make rich reading for the biologist, taxing him without bankrupting him. The gist of the argument is that the theory and practice of automata does in fact provide criteria of physical mechanisms in the light of present-day physics, against which biological systems can be tested. If this is not the case, Elsasser's approach loses much of its force. On this point, other readers who are unfamiliar with the field may be troubled by the impression that, while the major part of the argument is abstract and logical, another part seems to hinge on the natural history of electronic systems now in use. One wonders how sure the author can be that the boundaries of physical mechanisms are essentially fixed.

If we know what to expect of a mechanism, we have tests to apply to the various attributes of living organisms. It is clear that biological systems include a great many mechanisms. But the significant test in the author's opinion is given by those expressions of the biological world which may be formulated in terms of the acquisition, storage, and conversion of information. The simplest and most universal of these is the genetic system embodied in the chromosomes. The one that is superficially most similar in operation to the devices familiar to the physicist is the central nervous function of animals. The third—and here Elsasser touches on the sorest spot in mechanistic biology—is development and morphogenesis in those aspects classically covered by the term "epigenesis."

The outcome of his analysis is that living organisms, in these expressions, are not mechanisms as physics can define mechanisms. We need not consider the reasons why he arrives at this conclusion; it is not the function of a book review to give the plot away. But the trouble is not merely the usual "we can't explain this biological phenomenon physically because we don't know enough about it." We seem to know more than enough about the biological systems to prove that they do not conform, and the mechanistic unification can be saved only by discovering something about physical mechanisms that would remove some of their limitations. It is not that the physicist cannot design machines to perform functions analogous to those of organisms, but that the designs are so fundamentally different from those provided by organic evolution.

If we take the position that biological systems are a part of the physical world whose operations, in some very important respects, are not encompassed by the physicists' image of that world, the next step is clear: that image has to be broadened and the direction in which it should be broadened may be indicated by the problems of biology. In Elsasser's view, biology calls for extensions of physics, beyond quantum mechanics, in the same sense that the knowledge of subatomic phenomena called for advances beyond classical physics, without at all affecting the usefulness of the older science in a large domain for which it was designed in the first place. As to the direction in which the advance is likely to take place, his prediction sounds positively musical to a biologist's ear. He stresses the likelihood that the new physics, in one way or another, will be the physics of systems with complex structure. It takes no persuasion to convince a biologist that this indeed is the realm in which the answers to many of his problems lie; such a view has been forced upon him by experience. And if the realm needs to be defined otherwise than by its complexity, we may hazard that it is a domain of objects that are huge by the physicist's standards, with macromolecules at the lower limit. The fact that associations of large molecules are found in nature only to the extent that they are made by living organisms is probably not trivial.

Some of the theoretical implications of structural complexity are dealt with in a chapter on structure and variation. The final chapter deals with the theory of measurement.

If Elsasser is correct, the physical foundations of biology have yet to be established. Insofar as this book is a call to action, much of the action must take place in physics. The author is too modest about his acquaintance with the subject matter of biology to suggest how his ideas might influence the actual course of biological research. The earlier impact of physics upon biology has been fully assimilated. We need no longer be convinced of the merits of quantitation, the consolations of numbers, or the puissance of instrumentation. If the jinni does not appear when the biophysicist rubs his magic lamp, we are generally willing to buy him a bigger lamp. In any case, philosophy does not change the ways of scientists by direct precept, but only by influencing their thoughts about what they are doing. In this case we are assured of the validity of our mechanistic approach to parts of the biological whole. If physics does not sustain us in studies of the whole, we can still resort to the pure art of biological research, which consists of forcing organisms to tell us in their own terms the rules by which they operate.

DANIEL MAZIA
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Progress in Psittacosis Research and Control. F. R. Beaudette, Ed. Rutgers University Press, New Brunswick, N.J., 1958. xii + 271 pp.

This book is the record of the proceedings of the second symposium on psittacosis, held in New York City in February 1956. Both symposia (the first one was held in 1953) were carried out under the auspices of the New Jersey Agricultural Experiment Station, Rutgers University, and with the financial support of the Hartz Mountain Products Company of New York. The late F. R. Beaudette arranged both symposia. M. Pollard completed the editing of the proceedings of the second symposium.

Psittacosis is a public health and industrial problem which cannot be treated lightly, but which, at the same time, does not deserve the attention accorded a major problem. The purpose of this gathering was to provide suitable recommendations for its control. The symposium ended with the confident conclusion that psittacosis in parakeets and turkeys can now be controlled by adequate chemotherapy and isolation.

Although practical aspects are emphasized, the first three chapters—by Dubos, Huebner, and Shope, respectively—are stimulating lectures on general problems of host-parasite relationship. The most noteworthy contributions are those of K. F. Meyer and his associates on the ecology of avian psittacosis and on the chemotherapeutic control of psittacosis in parakeets. These chapters represent a wealth of experimental work exceedingly well planned and analyzed. Valuable information can be found in many of the other chapters. Steele and Scruggs describe recent epidemics, and Andrews discusses federal developments in psittacosis

control. The disease and its chemotherapy in turkeys are discussed by Davies and Delaplane, and chemotherapy in the parakeet is discussed by Bussell and Pollard. Psittacosis-like agents in mammals are described by Baker; various aspects of diagnosis, by Fagan, Volkert, Christiansen, Benedict, and Gordon. The book also contains the informal discussions which took place at the meeting and a chapter on current trends written by Pollard after the meeting.

The reader versed in the field of psittacosis will find a fair amount of repetition of materiai already available in other publications and some important omissions in the discussions, but he will find reading this book quite profitable. Although the scope of the book is rather limited, several chapters will enlist the interest of the student of the broad aspects of host-parasite relationship.

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Reports

Schedules of Irrelevant Signals and Maintenance of Monitoring Behavior

Abstract. Subjects clearly discriminated between critical and irrelevant signals, yet an intermittent schedule of irrelevant signals produced reliably higher rates of responding than did the continuous presentation of such signals. This is exactly the result obtained with similar variations in reward schedules. It seems that essentially "meaningless" changes in stimulation can reinforce behavior, too.

Engineering progress has increased the number of jobs in which the operator is primarily an observer. The important cues in these monitoring situations are slight changes in stimulation. Often these "critical signals" occur so infrequently that efficiency in detecting them is lowered after prolonged observation. One human engineering problem is to offset such effects.

Holland (*1*) reported that the rate and pattern of monitoring responses vary with the frequency and regularity of critical signals. The signals he used were of little significance to his subjects, which suggests that monitoring might be aided by the proper scheduling of artificially produced signals not crucial to the observer's main task.

For preliminary study of this possibility (*2*), we chose these variations in noncritical stimulation: none, constant, and intermittent. The hypothesis was that no variation (except for critical signals) and continuous variation in stimulation would quickly produce the same reduction in monitoring efficiency, whereas intermittent noncritical stimuli would produce a higher *rate* of observing responses (accuracy of signal detection was not studied).

Six male college students were paid to serve for six sessions; one subject became ill and missed his last two sessions, however. The subject faced an "instrument panel" containing two jewel lights (one not used) and three switches. Above this was a panel with a frosted-glass window in it that had a 1-in. red band at its far right side. In this apparatus, modified from that designed by Premack and Collier (*3*), a movement of switch No. 1 activated a projector shutter for approximately 1/5 sec; depression of either switch No. 2 or No. 3 would move another frame of a 16-mm film reel into position. (Depression of the right-hand button, that is, switch No. 3, both moved the film and lighted a green bulb.) The projector and shutter were inside a long, rectangular box, and threw an image onto the back of the frosted-glass screen.

There were three reels of film, each with some 3000 images of a single vertical line. On all films the line appeared on the far right side of the aperture (in the red band) on every 80th frame. On the "no-variation" film, the line was always projected in the same position—slightly to the left of the mid-line of the screen—except when it appeared in the "critical" (far right, or red) position. On the "constant-variation" film, the line never appeared in the same position on two successive frames; the line's position was determined from a list of random numbers (modified to exclude repetitions). On the "intermittent-variation" film, the position of the line shifted sporadically. Random digits determined the number of frames on which the film would then shift. The indicator remained in one place for an average of 6.50 exposures.

Each subject was told simply that his task was "to score as many points as possible" by reporting whenever the gauge-indicator "moved" into the red band. The impression was created that the indicator was moving continuously; actually, the film was moved by the subjects' working the switches. Subject reported critical signals (line in the red band) by pushing switch No. 3; this turned a green light on (which subject believed to be a confirmation of the accuracy of his report), and moved the film. Usually subject threw switch No. 1, looked at the indicator, then threw switch No. 2; after every 80th observation, however, he threw switch No. 3 instead of No. 2 (because a critical signal had occurred). All activations of switches No. 2 and No. 3 were totaled on a counter that was read every 60 seconds by the experimenter.

The subject viewed one reel per session, and each subject had a different order of presentation of the films over the first 3 days; this order was reversed for the last 3 days.

Table 1 shows the average rates of responding for each subject under each condition. Rates were highly stable within sessions, especially after the first 2 to 4 minutes, so this was not treated as a variable. Analysis of these rates (Table 2) reveals a reliable difference between treatments.

While the original hypothesis was only partially confirmed, the most important prediction was verified: intermittent presentation of irrelevant signals did facilitate observing-response rate. The unpredicted inferiority of the constant-variation schedule probably resulted because this condition required the subject to relocate the indicator position on every trial, so that switch No. 2 movements more often occurred slightly later

Table 1. Summary of mean rate of observing responses (in responses/min). Rates are based on performance during two 45-minute sessions, excepting those for subject F; this subject missed the final two sessions because of illness, so his scores for "no variation" and "intermittent variation" are from only one session each.

Subjects	Treatments		
	No variation	Intermittent variation	Constant variation
A	39.33	43.96	38.97
B	28.42	30.71	27.49
C	29.67	29.81	26.39
D	25.46	26.97	22.98
E	25.39	24.14	22.89
F	29.16	29.60	27.58
Mean	29.58	30.87	27.72
S.D.	5.09	6.80	5.81

Table 2. Analysis of mean rate of observing responses (in responses/min).

df	Mean square	*F*	*p*
	Between treatments		
2	15.025	12.60	.01
	Between subjects		
5	105.602	88.43	.001
	Treatments × *subjects*		
10	1.192		

Instructions for preparing reports. Begin the report with an abstract of from 45 to 55 words. The abstract should *not* repeat phrases employed in the title. It should work with the title to give the reader a summary of the results presented in the report proper. (Since this requirement has only recently gone into effect, not all reports that are now being published as yet observe it.)

Type manuscripts double-spaced and submit one ribbon copy and one carbon copy.

Limit the report proper to the equivalent of 1200 words. This space includes that occupied by illustrative material as well as by the references and notes.

Limit illustrative material to *one* 2-column figure (that is, a figure whose width equals two columns of text) or to *one* 2-column table or to *two* 1-column illustrations, which may consist of two figures or two tables or one of each.

For further details see "Suggestions to Contributors [*Science* **125**, 16 (1957)].

than they did under the treatments that had periods of no indicator movement. The small numerical differences between treatments and the large variability between subjects restrict the practical implications of these findings. Of greater significance is the similarity of these data to the results of certain experiments with other animals (*4*). Taken altogether, these studies suggest that many more conditions can sustain or even form habits than have traditionally been acknowledged by psychologists.

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27 October 1958

Biliary Excretion by the Rat of Bromsulfalein as a Conjugate of Glycine and Glutamic Acid

Abstract. Paper chromatograms of bile collected after the intravenous administration of bromsulfalein to rats reveal four distinct bromsulfalein bands. One migrates with the same R_f as standard bromsulfalein; the other three move less fast. The major band, which accounts for 71.3 percent of the injected bromsulfalein, appears to be a conjugate of bromsulfalein with glycine and glutamic acid.

Bromsulfalein (sodium phenoltetrabromphthalein disulfonate) (BSP) is one of a group of phthalein dyes that is removed from blood predominantly by the liver and excreted into the bile (*1*). Dye removal is impaired in the presence of hepatocellular damage, and BSP retention in blood has proved to be a sensitive index of hepatic dysfunction (*2*). Although this dye has been used extensively in the clinical detection of hepatic disease, the precise mechanisms by which it is handled by the liver are still poorly understood.

A considerable body of evidence (*3, 4*) suggests that hepatic removal of BSP depends upon the simultaneous operation of at least two processes: (i) uptake of dye by liver cells until the cellular space is saturated with respect to a given blood level, and (ii) transfer from blood to bile by a rate-limited transfer mechanism. In order to further examine the process of biliary excretion, an analysis of BSP as it appeared in the bile of the rat was undertaken (*5*).

Fine polyethylene tubing was inserted into the common bile ducts of rats (Long-Evans, Wistar) under ether anesthesia, and bile was allowed to drain into small bottles while the rats were gently restrained in special cages. After collection of a control sample of bile, approximately 5 mg of BSP was injected intravenously, and additional bile samples were obtained. Aliquots of the bile were applied to Whatman No. 1 filter paper, and the chromatograms were developed in a descending system consisting of glacial acetic acid:water:*n*-propyl alcohol (1:5:10 vol./vol.). Usually four and occasionally three chromatographically distinct BSP bands were identified in bile by (i) the development of a purplish color on exposure of the paper to ammonia vapors, and (ii) absorption in the ultraviolet (Fig. 1). One of these bands, band D, migrated with the same R_f (0.75) as standard BSP, while bands A, B, and C moved less far, with average R_f's of 0.44, 0.51, and 0.60, respectively. When BSP was incubated with control bile for as long as 3 hours in vitro and the mixture was chromatographed, only a single band with the same R_f as standard BSP was observed (Fig. 1).

The distribution of BSP between the different bands in bile obtained from two rats is presented in Table 1. Bile was collected in a single tube for 150 and 180 minutes, respectively, after the intravenous administration of BSP, and aliquots were chromatographed; the bands were identified, cut out, and eluted with water; and BSP content was determined colorimetrically after addition of 20-percent KOH to appropriately diluted samples. It is apparent that band A contained most of the excreted BSP, accounting for 71.2 and 71.4 percent, respectively, of the total BSP injected in these two rats. Bands B and D contained smaller amounts of BSP. Band C was identified in one of these specimens, and when observed in bile from other rats it contained only very small quantities of BSP, as judged from the intensity of the purple color which developed on exposure of the paper to ammonia.

Table 1. Distribution of BSP recovered in bile after intravenous injection.

Amt. injected (mg)	Amt. recovered in bile (% of amt. injected)				
	Band				Total
	A	B	C	D	
*Rat No. 6-21** (wt. 312 g)					
5.13	71.2	12.8		11.9	95.9
Rat No. 7-1† (wt. 342 g)					
5.90	71.4	6.0	4.0	10.2	91.6

* Bile collected for 150 minutes.
† Bile collected for 180 minutes.

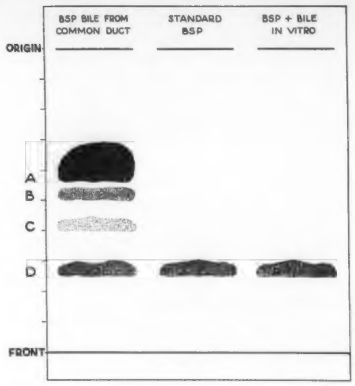

Fig. 1. Descending chromatogram on Whatman No. 1 filter paper in glacial acetic acid:water:*n*-propyl alcohol (1:5:10, vol./vol.).

The compound comprising band A has been subjected to further analysis, which indicates that it is a conjugate of BSP with the amino acids glycine and glutamic acid. Ninhydrin-reacting material which conformed both in shape and position to the ammonia reaction of band A was seen on chromatograms developed in the one-dimensional descending system (acetic acid:water:*n*-propyl alcohol) and a two-dimensional ascending (phenol-NH_3 and 80-percent pyridine) system. When band A was eluted from paper and subjected to acid hydrolysis in 5.9*N* HCl for 3 hours at 15 lb pressure, it yielded bands corresponding to those of free BSP (*6*), and two Ninhydrin spots. The latter were identified as glycine and glutamic acid from the following observations: (i) the Ninhydrin spots assumed the positions of glycine and glutamic acid on two-dimensional chromatography in phenol-NH_3 and 80-percent pyridine; (ii) when the unknown compounds were mixed with known samples of glycine and glutamic acid and chromatographed in the two-dimensional system, only two bands were seen, and these corresponded to the usual location of glycine and glutamic acid standards in this solvent system; (iii) the dinitrophenyl (DNP) derivatives of the unknown amino acids moved with the same R_f's as known DNP-glycine and DNP-glutamic acid in tertiary amyl alcohol-phthalate buffer (*p*H 6.0) (*7*).

The possibility remained that the glycine and glutamic acid were not conjugated with BSP but appeared in bile either as free amino acids or as a dipeptide that migrated to the same position as band A. Indeed, when control bile is chromatographed, Ninhydrin-reacting material with the same R_f as the BSP band may be identified in some speci-

mens. However, this material can be removed by eluting it from paper and passing it through a Dowex 50 – × 8 200-400 mesh column. Compound A is not affected by this procedure.

A visual comparison with known standards of the size of the spots and the intensity of the Ninhydrin color developed by glycine and glutamic acid derived from the BSP conjugate suggests that these amino acids are present in equimolar concentration and that for each mole of BSP there is a mole of glycine and a mole of glutamic acid. Chemical analyses to confirm this impression are now in progress.

Tests on band A for free or combined sulfhydryl groups were negative. It should be mentioned that no evidence of a BSP-glucuronic acid conjugate was found. Thus, the quantity of hexuronic acid (*8*) in compound A did not differ significantly from that contained in control bile migrating with the same R_f as A, and after incubation of A with β-glucuronidase (Worthington) at 37°C, *p*H 4.9, for 1 hour, no free BSP was detected by paper chromatography. Under similar conditions, the activity of the enzyme was demonstrated by its capacity to liberate phenolphthalein from phenolphthalein glucuronide.

The results of this study indicate that BSP is excreted in the bile of the rat as at least three and perhaps four compounds. This finding introduces a new complexity in the interpretation of the values for BSP T_m obtained in previous investigations (*4, 9*). Any analysis of biliary secretory T_m of BSP must henceforth take into account the probability that more than one transport process is operative in the movement of BSP from blood to bile.

The primary purpose of this communication is to describe the nature of the major excretory product of BSP in bile. This compound and its hydrolytic products were subjected to both chromatographic and chemical analysis. The results of these studies indicate that BSP is excreted by the liver of the rat primarily as a conjugate of glycine and glutamic acid, over 75 percent of the excreted BSP being found in this compound.

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6. Standard BSP, when subjected to acid hydrolysis and then chromatographed in the acetic acid:water:*n*-propyl alcohol solvent system, yields two BSP bands, one migrating with the same R_f as BSP, the other moving faster. These two bands are also identified after A is hydrolyzed.
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* Established investigator of the American Heart Association.

18 August 1958

An Artificial Reagent for the Diagnosis of Classical Hemophilia

Present methods for the laboratory diagnosis of classical hemophilia [hemophilia A, or antihemophilic globulin (AHG) deficiency] are modifications of the thromboplastin generation test (*1*) or are dependent upon the use of plasma from a patient with classical hemophilia as reagent (*2*). The first of these methods, although of confirmed value, is time-consuming; a disadvantage of the second method is that adequate amounts of hemophilic blood are not always easily available. For these reasons an alternative rapid method in which readily available materials are used would be of value.

The principle of an alternative method was suggested by the observations (*3*) of the effects of disodium ethylenediaminetetracetic acid (Na_2-EDTA) on certain clotting factors; rapid and pronounced losses of proaccelerin (factor V) and AHG have been the most marked findings (*4*).

Normal human blood was collected into Na_2-EDTA (*5*) (150 mg/100 ml of blood). After centrifugation the plasma was decanted and stored at +4°C for 30 days. Plasma so treated retained normal activities of plasma thromboplastin component (PTC), plasma thromboplastin antecedent (PTA), Hageman factor, prothrombin, proconvertin [factor VII, or serum prothrombin conversion accelerator (SPCA)], Stuart factor, and fibrinogen; however, AHG and proaccelerin fell to undetectable levels (< 0.1 percent). Such plasma was stored at −20°C in small aliquots until needed. Optimal quantities of Asolectin (*6*) and barium sulfate adsorbed oxalated bovine serum were added to such plasma immediately before the test, as sources of platelet prothromboplastic-factor–like activity and of accelerin, respectively. The buffer for all dilutions was Veronal acetate, *p*H 7.3 (*7*).

In a series of 12- by 75-mm tubes were placed 0.1 ml of this aged Na_2-EDTA plasma; 0.1 ml of Asolectin (0.005 percent); 0.05 ml of barium sulfate adsorbed bovine serum, diluted 1 to 80; and 0.05 ml of either normal or unknown plasmas in serial dilutions. The coagulation times for these mixtures after recalcification with 0.1 ml of 1/70*M* $CaCl_2$, were recorded, and the values for tubes containing the unknown plasma were compared with those for tubes containing serial dilutions of normal plasma (see Table 1). The average recalcification time for such a system, when buffer is substituted for the unknown plasma, is 14 minutes; the time is shortened to 5 minutes or less (normal, 4¼ minutes) when any one of the following is added instead: platelet-poor normal plasma; plasma from a patient receiving Dicumarol; Seitz-filtered normal plasma; barium sulfate adsorbed normal plasma; and plasmas from patients congenitally deficient in PTA (one patient), Hageman factor (one patient), PTC (three patients), proconvertin (one patient), and proaccelerin (one patient). However, in none of six patients diagnosed by the method of Soulier and Larrieu (*2*)

Table 1. Coagulation time with artificial reagent to which the various plasmas listed were added prior to recalcification.

Plasma	Av. time (min)
Normal, platelet-poor	4¼
Normal, Dicumarol	3
Normal, Seitz-filtered	3¾
Normal, $BaSO_4$-adsorbed	2½
From PTA-deficient patient	4¾
From individual with Hageman trait	3¾
From PTC-deficient patient No. 1	4¼
From PTC-deficient patient No. 2	4¼
From PTC-deficient patient No. 3	4½
From proconvertin-deficient patient	2½
From proaccelerin-deficient patient	5
From AHG-deficient patient No. 1	6¼
From AHG-deficient patient No. 2	7
From AHG-deficient patient No. 3	8
From AHG-deficient patient No. 4	8
From AHG-deficient patient No. 5	10¼
From AHG-deficient patient No. 6	15
Veronal-acetate buffer (*p*H 7.3)	14

as cases of classical hemophilia was the prolonged recalcification time corrected to normal in the above system.

It would thus appear that the diagnosis of classical hemophilia can be made with this artificial reagent; diagnosis by this means would have the advantage of simplicity and availability of material (*8*).

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15 September 1958

Fusion Contour for Intermittent Photic Stimuli of Alternating Duration

Abstract. It is generally thought that fusion of intermittent photic stimuli occurs when the duration between successive pulses of light is reduced to a certain value, this value being a function of the illumination and viewing conditions. The findings described in this report show that fusion is determined not only by the duration between successive stimuli but also by the temporal pattern of successive stimuli.

Most studies of visual flicker are made with intermittent photic stimuli of equal duration repeated serially (Fig. 1, *a* and *b*). Although such stimuli are usually described in terms of their frequency, the reciprocal of frequency, or period, may also be used for this purpose. As the frequency of an intermittent light is increased (that is, as its period is decreased) the apparent rate of flicker increases until a point is reached at which the light ceases to flicker and appears steady. The frequency at which this fusion occurs is called the critical flicker frequency.

The critical flicker frequency is usually taken to be a measure of the temporal resolving power of the eye. One explanation of fusion (*1*) is that at the critical flicker frequency the excitatory effects of one stimulus persist until the arrival of the next stimulus, providing a constant excitation process. A second explanation of fusion (*2*) is that there is a refractory period after each stimulation. Any stimulus arriving during this refractory period will fail to result in excitation. A third explanation (*3*) involves the threshold of brightness discrimination. Stimulation by a flash of light raises the excitation level to a certain value. When the light is turned off, the level of excitation starts to decline. The onset of the next flash of light raises the excitation level to its previous value. At the critical flicker frequency, successive pulses of light occur so rapidly that the excitation level fluctuates between limits which are not detectably different in brightness. All three explanations assume that for given viewing conditions fusion will occur whenever the duration between successive stimuli is less than a certain value.

Recently we have been studying visual flicker with intermittent photic stimuli of unequal periods (Fig. 1, *c*). Note that the train of pulses shown in *c* does not represent the mixing of two independent frequencies. With such stimuli flicker may be obtained even though the duration of each of the alternating periods is less than the duration of that period which, when repeated serially, is seen as fused. For example, if both *a* and *b* in Fig. 1 are above the critical flicker frequency, when alternated serially, as in *c* of Fig. 1, they may appear to flicker. The purpose of this report is to present data on the fusion contour for intermittent photic stimuli of alternating duration (*4*).

"On" and "off" triggers generated by four variable-frequency oscillators produced electric square waves which were the input to a Sylvania R1131C glow-modulator tube. The rise and decay time of the tube is approximately 20 μsec. The four oscillators were set so that two of them determined the "on" time and the "off" time, respectively, of one period; the remaining two determined the "on" and "off" times of the second period. They were then connected in series to produce intermittent electric square waves composed of two alternating periods (as in *c* of Fig. 1). Either of the two periods could be varied independently of the other. Within each period, "on" time always equaled "off" time. The glow-modulator tube illuminated a spot which subtended ½° of visual angle. The luminance of the test spot was approximately 1800 millilamberts (mlam); the luminance of the surround was approximately 22 mlam. All observations were made monocularly.

Measurements were obtained in the following manner: One period (*A*) was set at a fixed value while the other period (*B*) was varied by the observer until a fusion point was obtained. Measurements were first made with *A* set to a value of 2 msec. In successive measurements, the period of *A* was increased in 2-msec steps until it reached a value for which no further fusion points could be found.

Figure 2 shows the data for two subjects. The coordinates of Fig. 2 have been labeled in terms of both period and frequency. The frequency label has reference only to the frequency which either period would have if it were pre-

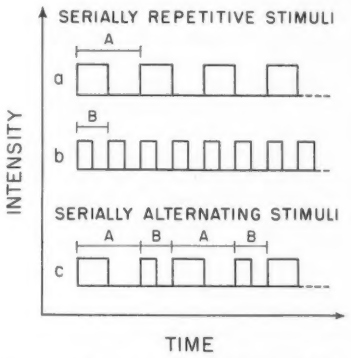

Fig. 1. Schematic representation of intermittent photic stimuli used in studies of visual flicker.

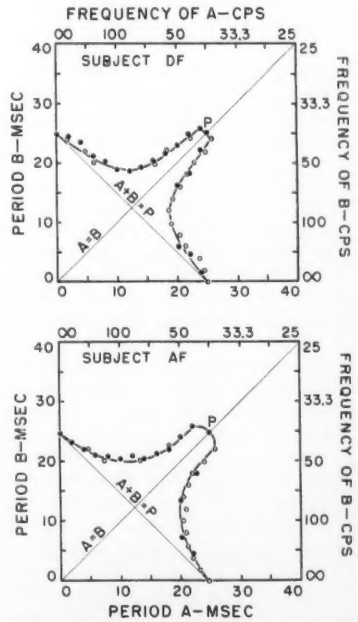

Fig. 2. Fusion as a function of the duration of alternate periods of intermittent photic stimulation. The solid circles are fusion points obtained by varying the period of *B*, with *A* constant. These points are based on ten observations per observer. The open circles are their mirror images.

sented as in *a* or *b* of Fig. 1. Each circle represents the values of two periods, *A* and *B*, which when alternated serially appear fused. Since the labeling of either period in such a combination is arbitrary, the circles below the diagonal line $A = B$ are mirror images of those above the line, and vice versa. The solid circles in Fig. 2 represent the fusion points obtained by varying period *B* while the period of *A* remained constant. These points are based on ten observations per observer. The open circles are the mirror images of the solid circles.

The fusion contour formed by passing a curve through all the points contains the traditional critical flicker frequency at three points: where it intersects the line $A = B$ and at its intersections with the two axes. All other points on the contour are for combinations of two periods of unequal duration. All combinations of *A* and *B* within the area bounded by the contour and the two axes appear as fused. All combinations of *A* and *B* which lie outside this area appear to flicker.

An examination of Fig. 2 shows that for some values of one period there are three values of the other which lie at a transition point between fusion and flicker. Thus, fusion may be reduced to flicker by increasing the average rate of stimulation—that is, by decreasing the duration of alternate periods. Conversely, flicker may be reduced to fusion by decreasing the average rate of stimulation —that is, by increasing the duration of alternate periods.

Note that no point on the contour falls below the line $A + B = P$, where *P* is the period associated with the critical flicker frequency. Even though flicker may be perceived when the period of each alternating pulse is less than *P*, the duration *P* appears to be a limiting factor for the temporal resolution of intermittent photic stimuli.

It is clear that the fusion of an intermittent light source cannot be explained solely in terms of a minimum duration between periods of stimulation for given illumination and viewing conditions. Fusion is rather a complex function of the temporal pattern of successive stimuli.

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29 September 1958

Continuous Protein Synthesis in Nuclei, Shown by Radioautography with H^3-Labeled Amino Acids

Abstract. Radioautographic investigation of the cell nuclei of adult mice after injection of leucine-H^3, methionine-H^3, or glycine-H^3 shows a high uptake of tritium by chromatin material but not by nucleoli. It is concluded that protein synthesis occurs continuously within nuclear chromatin.

Radioautographs of tissues of rats given methionine-S^{35} revealed that this amino acid is continuously being incorporated into proteins in all cells (*1*). Both nucleus and cytoplasm are involved in amino-acid incorporation (*2*), and, within the nucleus, a major role has been attributed to the nucleolus (*3*). However, this conclusion was based on investigations of large, but perhaps nonrepresentative, cells (starfish and amphibian ovocytes and diptera salivary-gland cells) from animals given amino acids labeled with C^{14} or S^{35}. Since the high β-ray energy of these isotopes does not allow good radioautographic resolution but the low energy of tritium (H^3) does, tissues from mice injected with either leucine-, methionine-, or glycine-H^3 were investigated (*4*).

Thirty adult male C3H mice (26 to 32 g) were divided into three equal groups. The animals of each group received a single subcutaneous injection (5 μc/g of body weight) of one of the three amino acids: DL-leucine-4,5-H^3 (150 mc/mmole), DL-methionine-methyl-H^3 (7.2 mc/mmole), and glycine-2-H^3 (13 mc/mmole). Two animals of each group were sacrificed at 0.5, 4, and 35 hours and at 7 and 45 days. The tissues were fixed in Bouin's fixing fluid and processed through dioxan for histology; thus, free amino acids were removed (*1*). Six-micron sections stained with hematoxylin-eosin were radioautographed (*5*). Briefly stated, radioactivity distribution was similar with the three amino acids, and, in confirmation of previous observations with methionine-S^{35} (*1*), radioautographic reactions were found not only over cells elaborating protein secretions (pancreas, thyroid, and so on) or undergoing renewal (hemopoietic organs, intestinal crypts, and so on) but over all other cells inves-

Table 1. Grain count over cytoplasm, nucleus, and nucleolus after injection of tritium-labeled leucine, methionine, or glycine in adult mice.

Amino acid	Time interval (hr)	Grain count (per 4 μ^2)		
		Cytoplasm	Nucleus	Nucleolus
		Pyramidal cell (cerebrum)		
Leucine-H^3	0.5	2.53	1.80	0
Leucine-H^3	4	1.96	1.60	0.03
Leucine-H^3	35	1.73	1.52	0.15
Leucine-H^3	168	0.77	0.52	0
Leucine-H^3	1080	0.31	0.17	0
Methionine-H^3	0.5	0.23	0.15	0.03
Methionine-H^3	4	0.97	0.63	0
Methionine-H^3	35	0.43	0.62	0.16
Methionine-H^3	168	0.07	0.06	0.01
Methionine-H^3	1080	0.07	0.04	0
Glycine-H^3	0.5	0.18	0.12	0
Glycine-H^3	4	1.10	0.79	0.03
Glycine-H^3	35	0.80	0.90	0
Glycine-H^3	168	0.13	0.07	0.03
Glycine-H^3	1080	0.13	0.03	0
		Purkinje cell (cerebellum)		
Leucine-H^3	0.5	2.18	1.95	0.08
Leucine-H^3	4	2.60	1.83	0.03
Methionine-H^3	35	1.07	1.27	0
		Liver cell		
Leucine-H^3	35	1.77	1.08	0
Methionine-H^3	35	1.30	1.17	0
		Sertoli cell (testis)		
Leucine-H^3	35	1.05	0.84	0
		Spermatocyte (testis)		
Leucine-H^3	35	1.33	1.53	0.04

tigated. Furthermore, reactions overlay cytoplasm and nucleus.

Photographic grains were counted over cellular constituents within areas delimited by either one or three small squares of the Whipple ocular micrometer; in the optical system used these areas measured 4 to 12 μ^2, respectively. The 4-μ^2 area was positioned over the central portion of nucleoli (2 to 3 μ in diameter) in large neurones, liver cells, and spermatocytes; the 12-μ^2 area was employed for the rest of the nucleus and for cytoplasm, and the background grain count (averaging 0.03 per 4 μ^2) was subtracted.

Each recorded figure (Table 1) is the mean of 20 corrected counts (ten cells in each of two animals). The results (Table 1) revealed that radioactivity appeared in the cytoplasm and nucleus of pyramidal cells soon after injection and eventually decreased with time, but was still present at 45 days. The rates of uptake and decrease gave no indication of an interrelationship between nucleus and cytoplasm; both seemed to incorporate amino acids simultaneously and independently. The other cells investigated also showed radioactivity in nucleus and cytoplasm. In contrast, the nucleolus was negative in most cells (Table 1). The few recorded counts may be due to a low degree of amino-acid incorporation into the nucleolus or, since the grains were often located at the periphery of the nucleolus, to uptake by the perinucleolar structures.

The nuclear radioactivity was analyzed by means of drawings of radioautographs of Purkinje cells in which chromatin masses were distinct. In six such drawings, at 35 hours after leucine-H^3 injection, no grains were found over the nucleolus; the grain counts over cytoplasm, nuclear sap, and chromatin were 1.0, 0.1, and 1.6 per 4 μ^2, respectively (the areas were measured by the "paper cut-out" method). Thus, maximal concentration occurred over chromatin. Indeed, grains overlay scattered chromatin masses as well as the nucleolus-associated chromatin. Similar observations at other time intervals and in other cell types confirmed the finding that most, if not all, nuclear radioactivity is located within chromatin masses.

Since chromatin masses consist chiefly of DNA and protein, associated to form chromosomes, the results demonstrated the uptake of three amino acids by chromosomal protein. Similarly, chemical studies by Allfrey *et al.* traced labeled amino acids into all protein fractions obtainable from nuclei, with a maximum in "residual proteins," which are believed to be the protein moiety of chromosomes (*6*). Furthermore, the finding of radioautographic reactions over chromatin material in all animals soon after injection (Table 1) indicated continuous uptake of amino acids by chromosomal protein, while the eventual decrease of the reactions with time (Table 1) indicated turnover.

Theoretically, the incorporation of amino acids into chromosomal protein may be due to adsorption, exchange, or synthesis (which may be total or partial). The maintenance of a reaction over a 45-day period would seem to eliminate the first possibility. The similarity of behavior of all three amino acids is believed to render the "exchange" possibility unlikely. Hence, the amino acids incorporated by cytoplasm and nucleus are believed to have been used for synthesis of local proteins. Therefore, the degree of uptake of leucine, methionine, and glycine reveals that little protein is synthesized from these amino acids in the nucleolus but that active protein synthesis occurs continuously and independently within cytoplasm and nuclear chromatin.

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8 September 1958

Effect of Temperature on Isolated Stretch-Receptor Organ of the Crayfish

Abstract. Abdominal stretch-receptor organs of *Astacus leptodactylus* are investigated by means of extra- and intracellular leads. The effects of changing temperature on steady-state rate of activity are surprisingly low. The sensory nerve cell compensates for changes in temperature by means of opposite reactions of generator potential and threshold.

Kerkut and Taylor (*1*), investigating the spontaneous activity of isolated ganglia of the slug, cockroach, and crayfish, found an "anomalous" transient response to changes in temperature. When temperature is increased there is a transient decrease in rate of activity, whereas if temperature is decreased, the activity shows a transient increase. The final rate of activity shows the normal temperature effect: the rate is faster at higher temperatures. The same type of response was reported by Florey (*2*) for stretch-receptor organs in crayfish. This response has now been more closely examined by means of intracellular leads, leading off from the sensory nerve cell.

The time course of spike frequency during and after sudden changes of temperature resembles very much that reported by Kerkut and Taylor for abdominal ganglia of crayfish. The temperature coefficient of activity depends on the stretch to which the receptor is subjected, but in general it is surprisingly low. The receptor may be stretched an additional 100 percent of its length in the relaxed state before impulses drop out [a state known as overstretch (see *3*)]. In the lower part of this working range the rate of impulses in the steady state is nearly independent of temperature, though immediately after a change in temperature a transient, anomalous response is seen. With the increasing length of receptors the Q_{10} increases; values of $Q_{10} = 1.5$ are common. Strongly stretched receptors, nearly in the state of overstretch, show higher values, but in no case has a Q_{10} above 2 been observed. Receptors that are completely relaxed but still spontaneously firing, or very gently stretched receptors, exhibit a Q_{10} below 1; here the steady-state rate of activity decreases with increasing temperature, as in the transient response.

It is thought that the surprisingly low values of Q_{10} and the diphasic time course of activity are both signs of an effect compensating for changes of excitability during changes of temperature. Experiments with intracellular leads have shown that with decreasing temperature the generator potential mediated by stretch increases. This means that the membrane potential attained after a stretch decreases with decreasing temperature. On the other hand, with decreasing temperature a decrease in the critical firing level of the membrane is observed. It is thought (see *4*) that the difference between depolarization of the membrane by generator action and the critical depolarization for spike activity determines spike frequency. This difference remains nearly independent of temperature, in the way mentioned above. The changes of generator potential and threshold after a sudden change in temperature are slightly different with respect to time course; this difference is responsible for the diphasic time course of spike frequency.

An interesting feature is the blockage of impulses by means of extreme temperatures. In the regular pattern of spikes, one suddenly drops out. In the regular place of a dropped-out full

spike, a miniature potential remains. This suggests that rhythmical spike activity is triggered by rhythmical local events in the dendrites or soma of this nerve cell (see *5*).

It seems to be of general interest that the time courses of activity in peripheral nerve cells and in central nervous tissue are similar after a change in temperature. This, together with the finding that the isolated nerve cell already is stabilized against changes of excitability by means of two interacting processes of opposite sign (see *6*), shows that there is a strong compensation for effects of temperature in cold-blooded animals (*7*).

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19 September 1958

Second Spectroscopically Abnormal Methemoglobin Associated with Hereditary Cyanosis

Abstract. Isolation of an abnormal methemoglobin from two families exhibiting dominantly transmitted cyanosis have permitted the recognition of two different pigments of the hemoglobin M type. It is possible that the abnormal properties which characterize the acidic methemoglobin derivatives result from a crevice configuration of the heme, with two Fe-protein bonds.

In a previous article (*1*) an account was given of the electrophoretic isolation of both a normal and an abnormal hemoglobin from an affected member of a family exhibiting dominantly transmitted cyanosis. The abnormal component was designated hemoglobin M because the absorption spectrum of the acidic methemoglobin showed the same anomalous features as the oxidized whole hemolysates from the patients examined by Hörlein and Weber (*2*), by Kiese, Kurz, and Schneider (*3*) and by Heck and Wolf (*4*). A second family, living in Canada, with a comparable genetic transmission of cyanosis (*5*) has now been studied, and the presence of a similar spectroscopically abnormal methemoglobin has been demonstrated (*6*). However, upon isolation the latter methemoglobin was found to differ from that present in the first family in several fundamental respects. In the following brief account, the clinical, spectroscopic and chemical features characterizing and distinguishing these two abnormal hemoglobins will be given. To facilitate discussion, the pigment described previously (*1*) will be identified as hemoglobin M, Boston type (symbolized as Hgb M_B), and that described here for the first time will be identified as hemoglobin M, Saskatoon type (Hgb M_S).

Clinically, the presence of either variety of hemoglobin M is accompanied by cyanosis. With Hgb M_B, an increased level of methemoglobin could not be demonstrated by the method of Evelyn and Malloy (*7*). The cyanosis occurring with Hgb M_S, on the other hand, was associated with an increased amount of methemoglobin as determined by this technique (*8*).

Separation of the hemoglobin into two fractions was effected in both types of patients by starch block electrophoresis of the hemolysates after conversion into methemoglobin by treatment with potassium ferricyanide (Fig. 1). Optimum resolution occurred under the conditions of cathodic migration (sodium phosphate buffer, *p*H 7.0, ionic strength 0.1). Each electrophoretic band was distinctively colored: methemoglobin A was brown, M_B was gray, and M_S was green. The several pigments were recovered in pure form by elution after careful excision of each colored band from the starch block. The eluates were then examined spectroscopically by one of us (P. G.).

The absorption spectra of the acidic forms of methemoglobins M_B and M_S from 450 to 700 mμ are shown in Fig. 2 (*B* and *C*). In both, the 632 mμ peak of acidic methemoglobin A (see Fig. 2*A*) is absent, and both are characterized by a new peak at 602 mμ. The intensity of this new maximum relative to the common maximum near 500 mμ is appreciably greater for methemoglobin M_S than for methemoglobin M_B, namely 0.72 compared with 0.61. Furthermore, in the spectrum of methemoglobin M_S there is a poorly resolved band at about 540 mμ which is scarcely perceptible in methemoglobin M_B. This same band is a little more pronounced in methemoglobin A. In the Soret region the maxima for acidic methemoglobin M_B and M_S are at about 406 mμ (like that of methemoglobin A), but both have lower intensities. Relative to the intensity of the Soret band for the corresponding carbonmonoxyhemoglobins, the values are 0.92 for Hgb A, 0.76 for Hgb M_S and 0.62 for Hgb M_B.

In the reactivity of their hemes there is an even more marked contrast: methemoglobin M_S resembles methemoglobin A, whereas methemoglobin M_B is in a class apart.

Methemoglobin M_S reacts rapidly with the ligands F^-, CN^- and N_3^- to give the usual complexes. It is reduced very rapidly by $Na_2S_2O_4$ and is oxidized by H_2O_2 to the higher oxidation state. All these reactions proceed smoothly to completion, and in this respect methemoglobin M_S is indistinguishable from methemoglobin A, although there may be differences between the rate constants which would become apparent in a detailed kinetic study. In addition, although acidic methemoglobin M_S has its own characteristic spectrum, in solutions of $pH > 9$ an alkaline form predominates

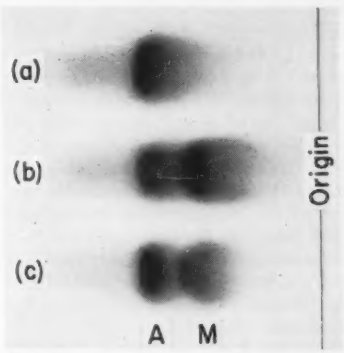

Fig. 1. Starch block electrophoresis of oxidized hemolysates (that is, the methemoglobins) at *p*H 7.0: *a*, normal blood; *b*, Hgb M_S trait; *c*, Hgb M_B trait. Migration is toward the cathode.

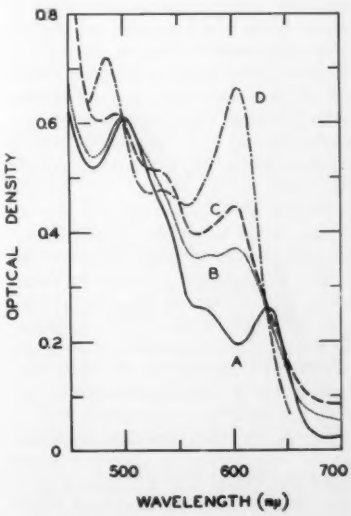

Fig. 2. Absorption Spectra at *p*H 7.0: *A*, methemoglobin A; *B*, methemoglobin M_B; *C*, methemoglobin M_S; *D*, methemoglobin A fluoride complex. For purposes of comparison all the optical densities have been made equal to 0.61 at 500 mμ.

with a spectrum very similar to that of alkaline methemoglobin A.

On the other hand, spectrophotometric measurements in the visible and Soret regions show that only a fraction of the hemes in methemoglobin M_B give an alkaline form or react rapidly with F^-, CN^-, and N_3^- to give the normal complexes, even though the same concentrations are used that give rapid and complete formation with methemoglobins M_S and A (*9*). Treatment with H_2O_2 gives about the same fraction of the higher oxidation state. Likewise, on addition of excess $Na_2S_2O_4$, about the same fraction undergoes rapid reduction. The remaining fraction is then reduced very slowly at *p*H 7, and a little faster at *p*H 10 with a half-reaction time of about 20 minutes at 25°C. Reduction in the presence of CO follows a similar course, but at *p*H 7 the remaining fraction reacts somewhat more rapidly with a half-reaction time of about 3 minutes. Analysis of these data suggests that half of the hemes react rapidly and the other half slowly, if at all. Partial denaturation cannot be the explanation of this remarkable behavior, because upon complete reduction at *p*H 10 the Soret spectrum has a single band with its peak at 430 mμ, characteristic of a true native hemoglobin.

The reactivity and absorption spectra of the hemolysates studied by Hörlein and Weber (*2*) suggest that their methemoglobin M component resembles methemoglobin M_S. With the exception of the spectrum in alkaline solution (*4*, Fig. 3*b*), the same inference would appear to hold for the single case described both by Kiese, Kurz, and Schneider (*3*) and by Heck and Wolf (*4*). However, until data on the isolated components are available no definite conclusion can be drawn, especially since other types of methemoglobin M may exist, different from both methemoglobin M_S and methemoglobin M_B (*9a*).

Several of the abnormal features can be accounted for by the hypothesis that in acidic methemoglobin M some of the hemes are bound in a crevice so that a group from the protein occupies the sixth coordination position of the iron instead of a water molecule, which is generally accepted for the structure of acidic methemoglobin A. This would explain why only the spectrum of the acidic form is different, since the atoms directly bonded to the iron would be the same in all other derivatives. For example, if L^- is the ligand and Fe^+ the ferriprotoporphyrin iron atom, complex formation and reduction of methemoglobin A would occur:

$$\text{Globin-Fe}^+(H_2O) + L^- \rightleftharpoons \text{Globin-FeL} + H_2O$$

$$\text{Globin-Fe}^+(H_2O) + e^- \rightleftharpoons \text{Globin-Fe}(H_2O)$$

whereas, if a group Y, which may be neutral or negatively charged, is bonded to the iron originally, the reactions would proceed:

$$\text{Globin-Fe}^+ - Y + L^- \rightleftharpoons Y\text{-Globin-FeL}$$

$$\text{Globin-Fe}^+ - Y + e^- + H_2O \rightleftharpoons Y\text{-Globin-Fe}(H_2O)$$

According to this hypothesis some similarity might be expected between the spectra of the acidic forms of methemoglobins M_B and M_S and one of the typical complexes of methemoglobin A. It is therefore interesting to note that the band maxima for methemoglobin A fluoride, which are characteristic of a class of complexes with high magnetic susceptibilities, occur at almost identical wavelengths (see Fig. 2*B*, *C*, and *D*).

But even if the participation of crevice bonding is accepted in principle, the contrast between methemoglobins M_B and M_S is an indication of other fundamental structural differences. In methemoglobin M_B the fraction of the hemes that react rapidly may be bound normally as in methemoglobin A, while the fraction that reacts very slowly may be bound in a crevice deep within the polypeptide chains. With methemoglobin M_S, the more abnormal spectrum suggests as one possibility that a greater fraction, if not all of the hemes, are bound in a crevice configuration: yet the rapidity with which they react would require the bonding to be far more labile.

The hypothesis that the heme is situated in a crevice in normal hemoglobin has also been widely discussed, and is supported by indications of steric hindrance in the formation of its isocyanide complexes (*10*), and by more recent physical studies employing nuclear magnetic resonance (*11*). Other evidence, however, which has been surveyed in a recent review (*12*), would suggest that any such crevice configuration in normal hemoglobin enfolds the heme to a far lesser extent than the crevice present in cytochrome *c*. Furthermore, apart from the possibility of linkage via the porphyrin side chains, there is no evidence that the heme is held by more than one Fe-protein bond. X-ray studies favor a structure of this kind for myoglobin (*13*). In contrast, as proposed above, the distinguishing feature of acidic methemoglobin M, especially with the type designated Hgb M_B, may be a crevice configuration with two Fe-protein bonds (*14*).

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14. Part of this work has been supported by grants from the National Science Foundation (G-2309), and from the National Institutes of Health (H-2405). One of us (P.S.G.) is a Public Health Service research fellow of the National Heart Institute.

21 July 1958

Field Observations on Effects of Alaska Earthquake of 10 July 1958

Abstract. The Alaska earthquake of 10 July 1958 was caused by movement on the Fairweather fault amounting to at least 21½ feet horizontally and 3½ feet vertically. Effects of strong shaking were evident over a large area in southeastern Alaska. In Lituya Bay an enormous wave, possibly resulting from a rockslide, reached a maximum height of more than 1700 feet.

Late on the evening of 9 July 1958, local time, a major earthquake was felt at most of the principal communities in southeastern Alaska and in adjoining parts of British Columbia and Yukon Territory, Canada. The U.S. Coast and Geodetic Survey has made the following determinations: instrumental epicenter, at 58.6°N, 137.1°W [in the Fairweather Range of the Saint Elias Mountains, about 100 miles west of Juneau (*1*)]; origin time, $06^h15^m51^s$ Greenwich Civil Time, 10 July 1958 (*2*). Pasadena reports Richter magnitude $M = 8$; Gutenberg unified magnitude $m = 7½$ (*3*). On Khantaak Island near Yakutat, a

beach sank, carrying three persons with it, and in Lituya Bay an enormous wave killed two persons and destroyed two fishing boats.

The disturbance was strongest along the Pacific coast from Cape Spencer to Yakutat Bay; this suggests that the shock was initiated by movement along the Fairweather fault (*4*). An aerial and ground investigation of the epicentral area was started on the day after the earthquake and was continued on several days during August and early September.

The Fairweather fault, as recognized from geologic and geomorphic evidence, extends from Palma Bay at least as far northwest as the latitude of Nunatak Fiord, a distance of 115 miles. The effects of strong shaking were manifest over this entire distance; however, the surface trace of the fault is exposed in bedrock or reasonably competent mantle-rock for a total distance of only 6 miles in the vicinity of Crillon Lake and La Perouse Glacier. Elsewhere the fault trace is covered by ice, water, or recent alluvial deposits, so that the total length of the surface break cannot be determined.

A displacement in which the southwest side moved relatively northwest 21½ feet and up 3½ feet was found at one point on the Fairweather fault just east of the north end of Crillon Lake. This displacement was measured between the offset ends of a straight band of grass, near the bottom of a minor depression that crosses the fault line at a high angle. The zone of shattered soil and rock here is about 6½ feet wide and strikes N 41°W. The attitude of gouge marks or striations on a slickensided scarp near this locality substantiates the determinations of direction of relative movement and ratio of horizontal to vertical components of movement measured from the band of grass. At most places the breakage zone was found to be wider than 6½ feet and braided in appearance, and it was found to consist of two or more subparallel furrows of disturbed soil that had numerous cross breaks between and outside them.

Other displacements of right lateral movement ranging from 8 to at least 11 feet were measured between the offset segments of trees that had fallen prior to the earthquake and were partly imbedded in the soil across lines of breakage. Unfortunately, none of these trees completely spanned the entire rupture zone, so these measurements indicate only a part of the total displacement on the fault.

On bedrock ridges east of the center of Crillon Lake, flat areas on both sides of the Fairweather fault and ½ to ¾ mile from it were extensively shattered by vertical displacements along planes that strike parallel or at a low angle to the main fault. In both areas the net total relative displacement is up on the side toward the Fairweather fault. The vertical displacement makes the surface breakage in these areas look more spectacular than the breakage resulting from the larger but predominantly horizontal movement in the main fault zone.

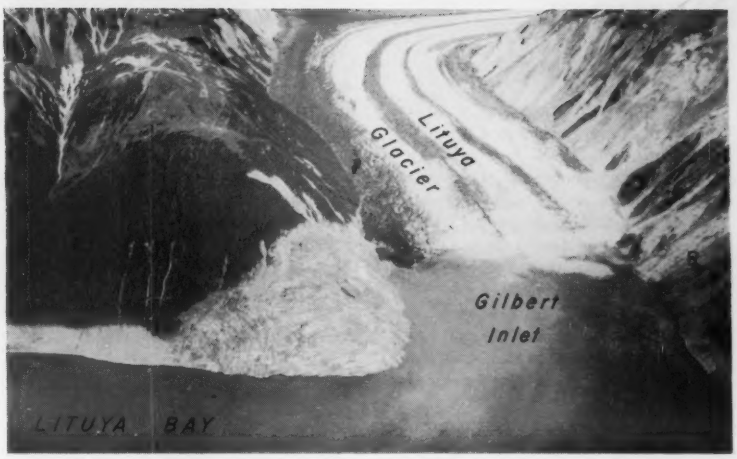

Fig. 1. Aerial view, looking north, at the head of Lituya Bay. A wave originating in the vicinity of a rockslide (*R*) in Gilbert Inlet destroyed the forest to an altitude of more than 1700 feet on the spur (left center). The trace of the Fairweather fault is covered by Gilbert Inlet and Lituya Glacier.

Earth slumps, lurches, rock and soil avalanches, rockslides, earth flows, and minor cracks and fissures were observed over a large area. Slumps, avalanches, and rockslides were most numerous from Lituya Bay south, although subsidence of the beach at Point Turner on Khantaak Island, Yakutat Bay, apparently resulted from a largely submarine slump. Mud and sand erupted from vents and fissures in soft water-saturated deposits at many places from Yakutat Bay southeast to Cross Sound.

In Lituya Bay the earthquake was followed almost immediately by an enormous water wave that originated in Gilbert Inlet, one of two arms along the Fairweather fault at the head of the bay. The wave was witnessed by fishermen on boats anchored in the bay. It resulted in the almost complete destruction of the forest over an area of nearly 4 mi² along the shores of Lituya Bay. The trimline, or upper limit of near-total destruction of the forest, left by the 10 July wave decreases in altitude from the head to the mouth of the bay, but at most places it is much higher than similar trimlines left by earlier waves (*5*). On the steep spur separating Gilbert Inlet from the main part of Lituya Bay, the water rose to an altitude of more than 1700 feet, stripping bare to bedrock a triangular area about 1 mile wide at the base (Fig. 1). At the apex large trees were washed out and turned uphill into the undamaged forest. A large proportion of the trees felled by the wave were stripped of limbs, roots, and even bark. The earthquake triggered a large rockslide, which plunged into Gilbert Inlet opposite the highest point on the trimline. The information now available indicates that this rockslide, alone or in conjunction with fault displacement, was probably the cause of the 1958 wave in Lituya Bay.

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References and Notes

1. Localities mentioned in this report are shown on U.S. Geological Survey maps of the Mt. Fairweather and Yakutat quadrangles (Alaska Reconnaissance Topographic Series; scale, 1:250,000).
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1 December 1958

Meetings

Collegiate Academy of Science

Developments during the past few years have aroused an interest in science and scientists which has become national in scope. Many people believe that our country fails to secure the full benefit of many of its gifted young people because these superior students do not obtain the education requisite to reach the level for which they are qualified.

Only about six out of ten of the top five percent of high school graduates ever earn college degrees. Why is this true? The decision to continue in college depends upon several factors, one of which is motivation. Talented students do not want to do just what everyone else is doing—they want an opportunity to do something creative. Are our college science students being properly motivated? Are there opportunities for a talented college student who is a potential scientist to act like one? One organization formed to stimulate superior undergraduate students to do independent scientific research is the Collegiate Academy of Science. The collegiate academy not only offers a stimulus to increase interest in science, but also provides for the undergraduate a medium through which he can publish his results. Through its annual meeting, the organization also offers students an opportunity to prepare and read scientific papers.

The members of the Academy Conference Committee on Collegiate Academies of Science believe that such an organization is a distinct need. Students sometimes come from high schools that have active junior academies or science clubs to colleges that have no agency for encouraging and promoting their interest in science. The Collegiate Academy of Science stimulates continued interest in science and prevents much scientific talent from being lost. Therefore, the committee is of the opinion that the absence of collegiate academies in most of the state academies of science is a serious matter. It is hoped that the following information on the purposes, organization, and procedures of a collegiate academy will stimulate interest in this important group.

The purpose of a collegiate academy is to stimulate scholarship and research among the undergraduate students in the colleges and universities of the state who are interested in the sciences; to cooperate with the state academy of science and to aid in accomplishing the objectives of that organization; and to encourage and facilitate the exchange of information and ideas among students interested in the sciences.

Active members are usually members of clubs affiliated with the collegiate academy. However, undergraduate students in colleges and universities of a state where there is no affiliated club may also become members. Any undergraduate science club or society of a college or university of a state may affiliate with the collegiate academy by sending an application to the executive committee (or to some designated person). Annual dues for individual members should be about $1.00. These may be the only dues collected or there may also be dues for each affiliated club. In the past, some collegiate academy groups were supported by the state academy of science, but experience has proved that self-support, made possible by annual dues, is preferable.

The officers should include a president, a vice president, and a secretary. A treasurer and an editor could also be included. In some instances, a faculty member serves as treasurer. If there is a large number of affiliated clubs, it might be useful to divide the state into regions (as northeast, northwest, southeast, southwest) with a director for each region. The officers should be elected at the annual meeting from students who will be in college for one more year and should hold office for one year. The faculty sponsor (or counselor) may be appointed by the state academy of science, or he may be elected by the collegiate academy and be approved by the state academy. A collegiate academy committee composed of faculty members from several colleges may be appointed by the executive committee of the state academy of science to assist and advise the counselor. The executive committee should consist of the officers, the regional directors (if any), the faculty sponsor, and the collegiate committee (if any). The immediate past president might also be a member of the committee.

The annual meeting, which is the principal activity of the collegiate academy, should be held in conjunction with that of the state academy of science. Members should be encouraged to attend the general meetings of the senior academy. Regional meetings or other special meetings might be held at times and places determined by the executive committee. At the annual meeting, the most important part of the program is the presentation of scientific papers by student members. Interest may be stimulated by offering a small prize for the best paper. This may simply consist in having the prize-winning paper published in the senior academy journal. In some cases, certificates of merit are awarded to authors of outstanding papers, while in other instances cash prizes are given. In any case, the greatest benefit to the student comes from the experience of preparing and delivering a scientific paper.

Other suggested activities for the an-

Kodak reports on:

the stylish advent of a new polyester structure . . . the Harrow touch in autoradiography . . . who can tell you how to speed up time

KODEL is the registered trademark for a new Eastman polyester fiber. Much is to be heard of it. It significantly extends the range of fabrics and apparel that can be classed as wash-and-wear or easy care. Garments in which it is properly blended with other fibers stay fresh and neat under trying conditions. They have a pleasing, comfortable feel. Here are some 55% *Kodel*-45% worsted blends in which it can be enjoyed this coming spring and summer.

Timely's "Planet" suit, a welcome addition to the wardrobe of commuter or global traveler. The modified lounge coat with slim lapels is available in both three-button and two-button models. Your choice of seasonable colors. About $60.

Michaels-Stern has tailored this suit for the busy executive in both a bengaline plain pattern and a corded stripe, in a full range of sizes and colors. The name is "Whisperwate" and with good reason—for the ultimate in cool comfort.

The long, slim, straight look of Varsity Town's "Picture Suit" takes on added distinction from the fabric—with Kodel's soon-to-be-famous ability for holding a press, fending off wrinkles. Note the longer lapels, higher pockets.

To the suavity customary in those who model men's wear, our three gentlemen above add authenticity. Alan Bell (left, Ph.D., McGill University, 1937) and Charles Kibler (center, Ph.D., University of Virginia, 1939) directed the first synthesis of the *Kodel* polymer in the laboratories of Tennessee Eastman Company—a true linear polyester containing a chemical group not previously present in polyesters. Emmett V. Martin (right, Ph.D., University of California, 1932), now a physicist, once wrote a paper (*Plant Physiol., 10,* 613-636) entitled "Effect of Artificial Wind on Growth and Transpiration in Helianthus Annuus." (That's the common sunflower.) He led the way to the unique physical structure of the *Kodel* fiber.

Imported plate

Autoradiography—the locating of radioactive tracer substance through densities or tracks generated in adjacent photographic emulsion—bloomed into full flower among histologists and other life-scientists during the period 1946 to 1951. Today the talk has drifted from technique to its findings. Unable to contribute much to that conversation, we content ourselves preparing for the autoradiographers extremely high concentrations of silver halide in gelatin. These emulsions we coat in thin layers on 5μ to 10μ plain gelatin layers for mechanical support and deliver the combination on a separable sheet of cellulose ester or a glass plate.

The user removes the double layer from its film or glass base, floats it on water emulsion side down, and then lifts it up on the slide bearing his suitably stained radioactive specimen section. In a stream of cool air, he dries his preparation down and sets it aside in a light-tight box for as many hours or days as the radionuclide requires to register its image. Then he gives the whole slide its photographic processing. The overlying gelatin layer is, of course, easily permeated by the processing solutions. Finally his microscope shows him where in his specimen the radioactivity lodged.

It so happens that in Rochester we prefer to make the base of cellulose ester, while our British cousins of Kodak Limited in Harrow put the emulsion and gelatin on glass. Moreover the Rochester and Harrow emulsions differ, the autoradiographers tell us. Knowing that emulsion making encompasses almost as many subtle variables as sonata playing, we won't argue with them. Instead we are now importing the *Kodak Fine-Grain Autoradiographic Stripping Plates AR. 10* for those who prefer them to a 35mm x 5-ft roll of *Kodak Autoradiographic Permeable Base Stripping Film.* Both have a 5μ emulsion. "Under favourable conditions," says Harrow of the plates, "a resolution of two microns may be obtained."

Eastman Kodak Company, Special Sensitized Products Division, Rochester 4, N. Y., will be delighted to have as many dozen of these 4¼" x 6½" plates shipped to a local Kodak dealer as you can pay him for at $12.40 (list) per dozen. As for the domestic film at $15 (list) per roll, ditto, of course.

Months to seconds

A *Cine-Kodak Special Camera* made most of the movies you have ever seen of a bean developing into a bean plant or a cow pasture developing into a steel plant. The *Cine-Kodak K-100 Camera,* which lists from $299, can likewise be adapted for this technique. No great resources in mechanical skills or facilities are demanded.

Mr. Fred O'Brien, Eastman Kodak Company, 400 Plymouth Avenue North, Rochester 4, N. Y., can tell you how to go about it.

Prices shown are subject to change without notice.

Kodak
TRADEMARK

nual meeting are as follows: (i) a discussion of opportunities in the different fields of science, followed by a question period (one or more members of the senior academy may indicate the types of positions available, salaries, opportunities for graduate study, and other related points); (ii) scientific exhibits (both commercial exhibits and exhibits prepared by students); (iii) scientific films; (iv) field trips to local places of scientific interest; (v) a collegiate academy banquet at which the president gives an address; (vi) a social hour or cafeteria lunch together to give students from the various colleges an opportunity to become acquainted; and (vii) a business meeting for committee reports, election of officers, and so forth.

A collegiate academy publication is, next to the annual meeting, the best means for maintaining interest. This journal should be devoted largely to the publication of papers written by the collegiates. The publication may also include a column by the president, the faculty counselor, or both, in which they discuss items of interest to all of the collegiates. News from the various chapters may also be included.

The following are additional activities which have proved successful in some of the collegiate academies now in existence: (i) local meetings; (ii) regional meetings, similar to the annual meeting but on a smaller scale; (iii) meetings of the executive committee (including one such meeting held several weeks in advance of the annual convention and another at the time of the annual convention); (iv) circular letters sent occasionally to each chapter by the president or the faculty sponsor to help maintain interest; (v) requests by the faculty sponsor for senior academy members to serve as speakers for local chapter meetings during the year (several chapters in the same city or within a few miles of one another may hold occasional joint "academy night" programs at which a senior-academy member gives a talk); and (vi) field trips sponsored by the collegiate academy.

It is the hope of the committee members that this statement of ideas concerning the purpose of and suggested activities for a collegiate academy will prove helpful to many who may wish to develop such an organization.

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Forthcoming Events

March

11–14. American Cong. on Surveying and Mapping, Washington, D.C. (J. H. Wickham, Jr., 1959 ASP–ACSM Consecutive Meetings, 610 Montgomery St., Alexandria, Va.)

13–14. American Otological Soc., Hot Springs, Va. (L. R. Boies, University Hospital, Minneapolis 14, Minn.)

13–15. Alabama Acad. of Sciences, Auburn. (H. M. Kaylor, Dept. of Physics, Birmingham-Southern College, Birmingham, Ala.)

14–15. Southwestern Soc. of Nuclear Medicine, 4th annual, New Orleans, La. (S. B. Nadler, SSNM, 1520 Louisiana Ave., New Orleans 15, La.)

15–20. American College of Allergists, San Francisco, Calif. (M. C. Harris, 450 Sutter St., San Francisco.)

16–19. American Assoc. of Petroleum Geologists, Soc. of Economic Paleontologists and Mineralogists, 44th annual, Dallas, Tex. (W. A. Waldschmidt, AAPG, 311 Leggett Building, Midland, Tex.)

16–20. American Inst. of Chemical Engineers, Atlantic City, N.J. (F. J. Van Antwerpen, American Inst. of Chemical Engineers, 25 W. 45 St., New York 36.)

16–20. National Assoc. of Corrosion Engineers, 15th annual conf., Chicago,

Ill. (NACE, Southern Standard Bldg., Houston, Tex.)

16–20. Western Metal Exposition and Cong., 11th, Los Angeles, Calif. (R. T. Bayless, 7301 Euclid Ave., Cleveland 3, Ohio)

17–19. National Health Council, Chicago, Ill. (P. E. Ryan, 1790 Broadway, New York, 19.)

17–20. Organization of Research, 4th intern, symp., Milan, Italy. (I. Svergie, Produktivitetsnamnden, Linnegatan 87, Stockholm Ö, Sweden.)

18–25. International Social Science Council, 4th general assembly (by invitation), Paris, France. (C. Levi-Strauss, Secretary-General, International Social Science Council, 19, avenue Kleber, Paris.)

19–21. Society for Research in Child Development, NIH, Bethesda, Md. (Miss N. Bayley, Laboratory of Psychology, National Inst. of Mental Health, Bethesda 14, Md.)

19–22. International Assoc. for Dental Research, 37th general, San Francisco, Calif. (D. Y. Burrill, Northwestern Univ., 311 E. Chicago Ave., Chicago 11, Ill.)

20. New Jersey Acad. of Science, annual, New Brunswick. (H. L. Silverman, 361 Highland Ave., Newark 4, N.J.)

23–24. Theory of Fluid Flow through Porous Media, 2nd conf., Norman, Okla. (C. G. Dodd, School of Petroleum Engineering, Univ. of Oklahoma, Norman.)

23–26. Institute of Radio Engineers, natl. conv., New York, N.Y. (G. L. Haller, IRE, 1 E. 79 St., New York 21.)

24–27. American Meteorological Soc., general, Chicago, Ill. (K. C. Spengler, AMS, 3 Joy Street, Boston, Mass.)

27–28. Michigan Acad. of Sciences, East Lansing. (D. A. Rings, Univ. of Michigan, Dept. of Engineering, Ann Arbor.)

27–28. Pennsylvania Acad. of Sciences, Gettysburg. (K. Dearolf, Public Museum and Art Gallery, Reading, Pa.)

28. South Carolina Acad. of Sciences, Columbia. (H. W. Freeman, Dept. of Biology, Winthrop College, Rock Hill, S.C.)

29–3. Latin American Congress of Chemistry, 7th, Mexico D.F., Mexico. (R I. Frisbie, Calle Ciprès No. 176, Zone 4 Mexico, D.F.)

30–31. Third Teratology Conf., Portland, Ore. (D. L. Gunberg, Dept. of Anatomy, Univ. of Oregon Medical School, Portland.)

30–1. American Orthopsychiatric Assoc., San Francisco, Calif. (M. F. Langer, 1790 Broadway, New York 19.)

30–12. Bahamas Medical Conf., 7th, Nassau. (B. L. Frank, 1290 Pine Ave., W. Montreal, Canada.)

31–2. American Power Conf., 21st annual, Chicago, Ill. (N. S. Hibshman, AIEE, 33 W. 39 St., New York 18.)

31–2. Symposium on Millimeter Waves 9th, New York, N.Y. (H. J. Carlin, Microwave Research Inst., 55 Johnson St., Brooklyn 1, N.Y.)

31–5. International Committee of Military Medicine and Pharmacy, 21st session, Paris, France. (Comité International de Médecine et de Pharmacie Militaires, Hôpital Militaire, 79, rue Saint Laurent, Liège, Belgium.)

(See issue of 16 January for comprehensive list)

Equipment

The information reported here is obtained from manufacturers and from other sources considered to be reliable, and it reflects the claims of the manufacturer or other source. Neither Science *nor the writer assumes responsibility for the accuracy of the information. A coupon for use in making inquiries concerning the items listed appears on page 406.*

■ SURFACE-MOISTURE AND SURFACE-DENSITY GAGES use a probe containing a fast-neutron source for moisture testing and a probe containing a gamma-ray source for density measurement. To perform tests, the probes are placed in contact with the surface, and the scatter of the radiation by the material is measured. Calibration curves relate scatter to moisture or density. Probes are used interchangeably with a portable scaler. The measurements sample a hemispherical volume of material of depth varying from approximately 3 to 8 in. (Nuclear-Chicago Corp., Dept. 624)

■ CONTINUOUS-TAPE CARTRIDGE is designed to accept up to 100 ft of paper or plastic punched tape for computer and similar applications. The cartridge does not require a driving belt or other external mechanism other than the tape punch or reader itself. (Brooks Research, Inc., Dept. 627)

■ MCLEOD VACUUM GAGE reads with high sensitivity to 10^{-6} mm-Hg and moderate sensitivity in the 10^{-7} mm-Hg decade. The entire range of the instrument is covered in one quadratic scale from 2×10^{-3} to 1×10^{-7} mm-Hg. Error is within ±2.5 percent at the high-pressure end of the range. For safety, the mercury reservoir is set into a plaster of Paris base which in turn is encased in a metal container. (Consolidated Electrodynamics Corp., Dept. 629)

■ FREQUENCY DETECTOR produces an output of 0 to 500 μa d-c in response to input frequencies of 0 to 500 cy/sec. Accuracy is better than ±1 percent. Only static magnetic components are used, and the signal being measured is the sole source of power for the instrument. Plug-in design and hermetic sealing are used. Shock resistance is 100 *g*. (Acromag Incorporated, Dept. 631)

■ PLANIMETER is designed especially for integrating records produced on 4-in.-wide strip charts of the manufacturer's recorders. Either linear or square-root totalization is available by interchange of cams. The chart feeds at the rate of 0 to 12 chart-hr/min, set by a rheostat. A pointer is positioned over the record line by means of a manually operated knob. Totals accumulate on a five-digit manual-reset counter. Accuracy is ±1 percent of full-scale count in a 24-hr period. (Fischer and Porter Co., Dept. 634)

■ INFRARED SPECTROPHOTOMETER combines a replica grating and a single prism in a double-beam double-monochromator optical system. The instrument records automatically on a horizontal strip chart with variable abscissa and ordinate expansion. Scanning speed is variable; provision is made for repetitive scanning. Switching from double-beam to single-beam operation is conveniently accomplished. An automatic wave-member drive scans continuously from 650 to 4000 cm^{-1} and preserves linear wave-member presentation. Resolution is 0.3 cm^{-1} over the entire range. Wave-member accuracy is 0.5 at 670 cm^{-1} and 5 at 4000 cm^{-1}. Photometric accuracy is 0.1 percent for any one reading, single beam. (Beckman Instruments, Inc., Dept. 619)

■ TEMPERATURE INDICATORS are disposable paper strips that respond to temperatures by exhibiting an irreversible color change. Strips are available for temperatures in the range 100° to 490°F. Response time is less than 1 sec; accuracy is ±1 percent. (Paper Thermometer Co., Dept. 643)

■ HYPERTHERMAL WIND TUNNEL uses a plasma-jet head coupled to an evacuated chamber to produce thermal effects corresponding to those encountered at Mach 20. The plasma jet is formed by air heated in an arc chamber and produces a flow smoothed and shaped by a supersonic secondary nozzle, producing a test section diameter of 1 in. The equipment is capable of continuous operation. (Giannini Plasmadyne Corporation, Dept. 636)

■ MOTION ANALYZER provides digital output of coordinates of position on a film frame. A projected image is viewed on a screen 11 in. in diameter. After positioning the crosshairs and angle screen, the operator presses a button that causes the data to be read into a card-punch or printer. Readout is in increments of 0.001 in. at the magnified image, and angle is measured to 0.1 deg. (Vanguard Instrument Corp., Dept. 646)

■ CENTRIFUGES, available in three models, use a ball-disk drive system to obtain constancy of rotation, including wow and long-term drift, better than ±0.05 percent. Speed is continuously variable from 0 to 800 rev/min. Speed measurement, accomplished with a 600-tooth disk and a magnetic pickup, enables direct readout of arm speed accurate to ±0.01 percent. Accessories include closed television systems, high-pressure air and hydraulic systems, and servo control. Diameters of the three models are 30, 60, and 96 in. (Genisco Inc., Dept. 639)

JOSHUA STERN
National Bureau of Standards, Washington, D.C.

(*Continued from page 357*)

tures, explain and expound and guide their disciples. They are the salt of the earth. A student who has had more than one or two such teachers is indeed fortunate. But many professors become so involved in their researches that they find no time for effective teaching; others have really no inclination for teaching but carry on in order to have an opportunity to do research work.

This state of affairs deserves more than ever the serious consideration of those responsible for the administration of our colleges and undergraduate schools, especially in the physical and biological sciences. No matter how many papers he has authored, the chief function of a teacher is to teach. This is an art which, from the time of Socrates, no amount of learned papers has ever been able to replace.

VICTOR G. FOURMAN
Syntomatic Corporation,
New York, N.Y.

Detection of Sex-Reversed Human Beings

The finding by Barr and Bertram (*1*), and subsequently by others, that the nuclei of somatic cells may be used to determine genetic sex, and by Davidson and Smith (*2*) that polymorphonuclear leucocytes may be similarly used, has proved to be very useful in studies of sexual dysgenesis (*3*). By their use, many cases of gonadal dysgenesis have been shown to involve disagreements between the genetic sex and the sex of rearing. In all cases, the patients have been found because of their complaints with reference to faulty sexual development or functioning. Neither method for determining genetic sex has been used to detect completely sex-reversed individuals —that is, genetic females who have sired offspring, and genetic males who have given birth to offspring. This is not surprising. Such individuals would have no clinical complaints relative to their sexuality and thus would not be examined. Yet, as individuals with extreme forms of Klinefelter's syndrome (*4*), they may exist. In the following paragraphs a method for finding such individuals is proposed.

Genetic males functioning as females and mated to normal males would give rise to XX, XY, and YY offspring in the ratio of 1:2:1. Presumably the YY offspring would die *in utero*; hence the expected sex ratio would be 1 female to 2 males. The small size of human families makes it unprofitable to attempt to distinguish such a ratio from the expected ratio of 1 female to 1.05 males. On the other hand, genetic females functioning as males and mated to normal females would be expected to have only XX offspring—that is, only daughters. Families with large numbers of daughters and no sons can, of course, be readily distinguished from families with both sons and daughters.

The simplicity with which genetic sex may be determined makes it feasible to investigate the genetic sex of the fathers of families with, say, at least six daughters and no sons. (The probability that a family with six children would have no sons is ~ .013, on the assumption of chance distribution and a sex ratio of 1.05.)

I have such a study under way. I would appreciate information concerning families with large numbers of daughters and no sons.

ARTHUR G. STEINBERG
Department of Biology, Western Reserve University, Cleveland, Ohio

References

1. M. L. Barr and E. G. Bertram, *Nature* **163**, 676 (1949).
2. W. M. Davidson and D. R. Smith, *Brit. Med. J.* **2**, 6 (1954).
3. "Symposium on chromatin sex determination," *Trans. N.Y. Acad. Sci.* **20**, 493 (1958).
4. W. O. Nelson, *Fertility and Sterility* **8**, 527 (1957).

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Physiologist-Pharmacologist. Varied background in physiology, pharmacology and toxicology. Publications. University, private laboratory and industrial experience. Desires research position. Will consider research position involving small amount of teaching. Box 29, SCIENCE. X

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Information Scientists: To evaluate, abstract, and code scientific information. Training in biological or medical sciences. Interest in writing and ability to read one or more foreign languages useful. Abbott Laboratories, Employment Division, North Chicago, Illinois. 2/20, 27

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Literature Searchers and Writers with experience in preparing commercial literature in organic chemistry field. Permanent New York City positions with technical publisher. Noncontributory profit sharing pension plan. Box 22, SCIENCE. 2/6, 13

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Research Assistants with either the Master of Arts Degree or equivalent experience or the Bachelor of Arts Degree for interesting work in isotope, biology, biochemistry, or clinical laboratories doing cancer research. Graduate instruction available depending upon qualifications. Please submit credentials to Personnel Manager, The University of Texas, M. D. Anderson Hospital & Tumor Institute, Texas Medical Center, Houston, Texas. 2/20

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13 February 1959

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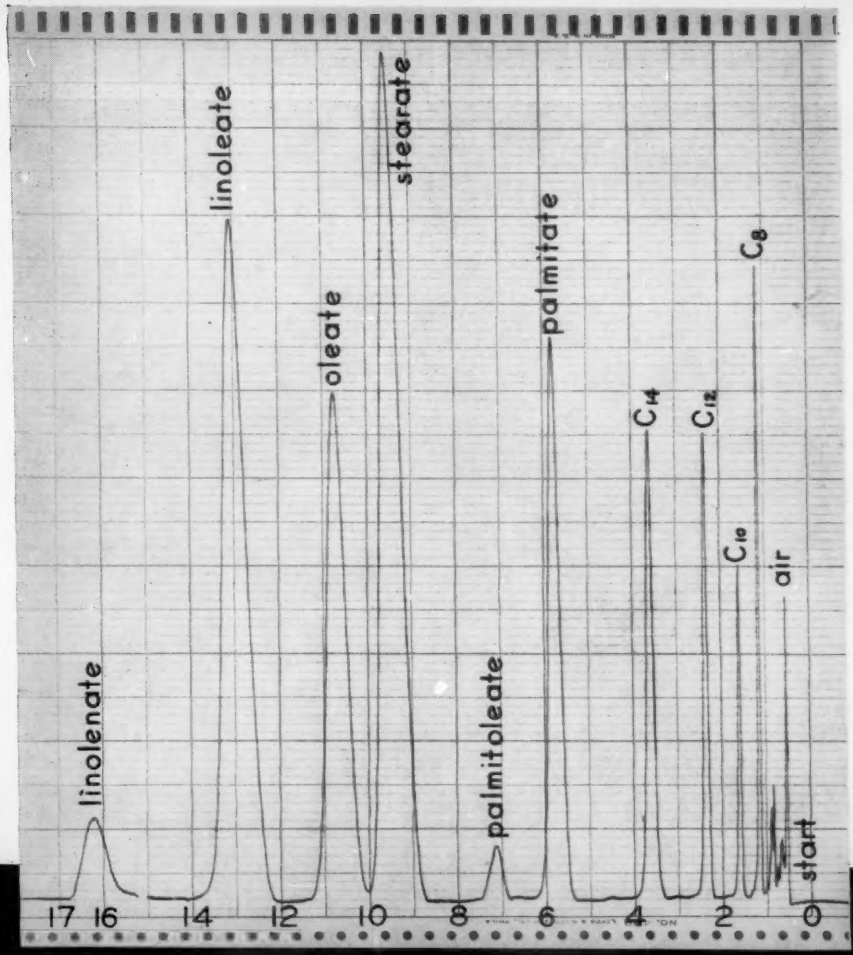

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*Hausdorff, H. H. and Brenner, N., "Gas Chromatography—Powerful New Tool for Chemical Analysis." *Oil and Gas Journal*, editions of June 30, July 14, July 21 and August 4, 1958.

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